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THE STEAMLOOP.

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[*A lecture delivered before the Franklin Institute, February 27, 1891.*]

The lecturer was introduced by the Secretary of the Institute, and spoke as follows:

MEMBERS OF THE INSTITUTE, LADIES AND GENTLEMEN:

When your Secretary honored me with an invitation to address you, he suggested the acceptability of remarks on centralization of power for distribution—electrically, hydraulically, pneumatically, or any way—over large areas, etc. I considered this proposition only to conclude that it was a little broad for my calibre, for I think my personal inclination in choice of subjects may have been bored out rather small originally, and possibly has been "bushed down" since. At any rate, I would regard it as taking an

unfair advantage to discuss ways and means I never tried; to quote authorities I never knew; to explain what is done abroad, where I have never been, and what is going to be done in a future, of which I am not the prophet. Therefore, I have selected a subject so simple that it can become for an hour a personal matter between ourselves and the blackboard. Novelty is its chief claim for attention, but in it there is, perhaps, enough to be learned concerning new applications of old laws to repay the time spent, enough by way of peculiar phenomena to excite admiration, and possibly—at first sight—a little that seems paradoxical.

The steamloop is generally called an ingenious device, and if so, the ingenuity certainly appears at once in the name; "Steamloop," a name which, though almost meaningless, seems very consistent with its simplicity. The name has the further merit of not portraying any of its functions or peculiarities, and hence cannot be an embarrassing restraint, as is so frequently the case with names attached to mechanical apparatus.

That so simple an application of Nature's laws as is involved in the steamloop should not have been turned to useful effect earlier is, at first thought, strange, but as one looks deeper into the subject, the reasons become more apparent. While no engineer is unfamiliar with the phenomena on which it depends, it has been interesting to note that even those best informed in practical steam engineering or theoretical research in thermo-dynamic science, seldom understand its action on first acquaintance, though they soon recognize in it a new combination of functions.

Its mission is the simple and useful one of returning water of condensation to steam boilers. Its chief characteristics are that its action is continuous, rapid and positive, and that it is a closed system operating under widely varying conditions, without valves or adjustments. Its construction is simply that of ordinary piping.

The principles on which its action depends are as follows:

Difference of pressure may be balanced by a water column.

Vapors or liquids tend to flow to the point of lowest pressure.

Rate of flow depends on difference of pressure and mass.

Decrease of static pressure in a steam pipe or chamber is proportional to rate of condensation.

In a steam current water will be carried or swept along rapidly by friction.

To these simple statements there will probably be no dissent. We have all used them in many ways, and some of them have disagreeably used us in a manner quite unwelcome. But it remained for the steamloop to collect a

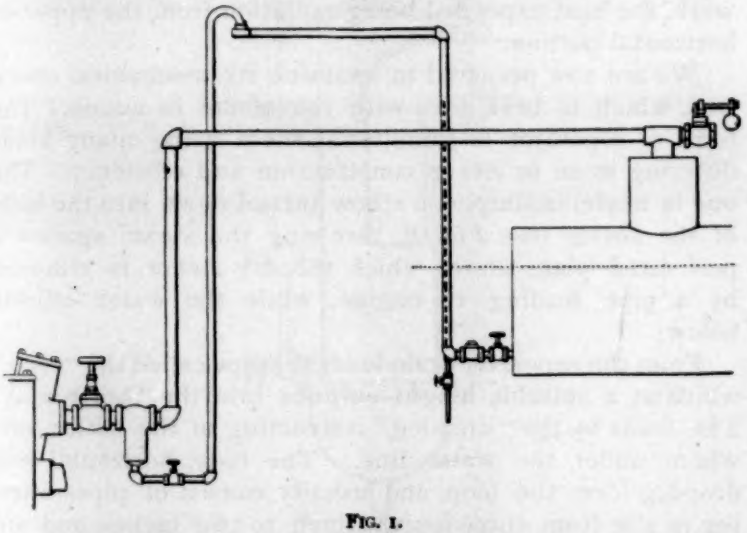


FIG. 1.

few of these erratic agents and from them create a useful system, combining the certainty of flow due to difference of pressure, with the quiet uniformity with which steam condenses, and with the force we see uselessly expended in the hammering of our steam-heating apparatus.

It will be evident that the steamloop, therefore, contains no mysterious factors, even though, like the steam injector, it has been called a paradox.

We have here a working model (see *Fig. 1*), the steam pipe passing from the boiler to a separator near the engine, which separates the water of condensation and entrainment

from the steam. The drip from the separator is below the boiler, and evidently were a pipe run from this drip outlet directly to the boiler, we would not expect the water to return up-hill. Moreover, the pressure in the boiler is (say) 100 pounds, while in the separator it is only ninety-five pounds, due to the decrease in pressure in the steam pipe by reason of which the steam flows to the engine. Thus the water must not only flow up-hill to the boiler, but also must overcome the difference in pressure. The device to return it must perform work, and in so doing heat must be lost. The loop therefore may be considered as a peculiar motor doing work, the heat expended being radiation from the upper or horizontal portion.

We are now prepared to examine its mechanical operation, which is best done with the model in action. The form of separator is immaterial, there being many kinds differing more or less in construction and efficiency. The one in model is simply an elbow turned down into the body of the device (see *Fig. 2*), throwing the steam against a perforated plate above which the dry steam is removed by a pipe leading to engine, while the water collects below.

From the separator drain leads the pipe called the "riser," which at a suitable height empties into the "horizontal." This leads to the "drop-leg," connecting to the boiler anywhere under the water line. The riser, horizontal and drop-leg form the loop, and usually consist of pipes varying in size from three-fourths inch to two inches, and are wholly free from valves, the loop being simply an open pipe giving free communication from separator to boiler. (For convenience stop and check valves are inserted, but they take no part in the loop's action.)

Suppose steam is passing, engine running and separator collecting water. The pressure of ninety-five pounds at the separator extends (with even further reduction) back through the loop, but in the drop-leg meets a column of water (indicated by the heavy broken line), which has risen from the boiler, where the pressure is 100 pounds, to a height of about ten feet. That is to the hydrostatic head equivalent to the five

pounds difference in pressure. Thus the system is placed in equilibrium.

Now, the steam in the horizontal condenses slightly, lowering the pressure to ninety-four pounds, and the column in drop-leg rises two feet to balance it, but meanwhile the riser contains a column of mixed vapor, spray and water, which also tends to rise to supply the horizontal as its steam condenses, and being lighter than the solid water of the drop-leg, it rises much faster. If the contents of the riser have a specific gravity of only ·1 that of the water in the drop-

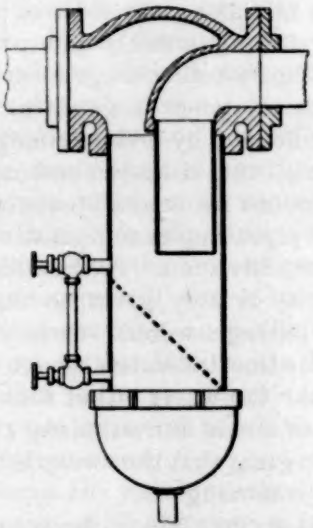


FIG. 2.

leg, the rise will be ten times as rapid, and when the drop-leg column rises one foot, the riser column will lift ten feet. By this process the riser will empty its contents into the horizontal, whence there is a free run to the drop-leg and thence into the boiler. In brief, the above may be summed into the statement that a decrease of pressure in the horizontal produces similar effects on contents of riser and drop-leg, but in degree inversely proportional to their densities. When the condensation in horizontal is maintained at a constant rate sufficient to give the necessary difference in

pressure, the drop-leg column reaches a height corresponding to this constant difference, and rises no further. Thus, the loop is in full action, and will maintain circulation so long as steam is on the system, and the differences of pressure and quantities of water are within the range for which the loop is constructed.

The above may be termed the rational explanation, but for simplicity we have omitted to include some of the features which are of importance in the loop's utility and which will appear later in our consideration of the subject.

"Solid" Water vs. Spray.—In the model the water sometimes appears as a film along the sides of pipes, or as spray, and often as slugs rising quickly and passing over. We cannot definitely state whether slugs form in practice in full size pipes, but their existence is doubtful except when the separator is partly flooded by boiler primage. In the model the pipes are so small that cohesion and capillary attraction are sufficient to account for the existence of the slugs. The resistance of small pipes causes comparatively slow velocity, and the slugs are readily seen. In the full-sized apparatus, however, the velocity is very great, so that even in glass sections a slug's passage would scarcely be discernible. Observations in this line indicate absence of slugs and confirm the belief that the water (other than spray) is carried chiefly by friction of steam current along the sides of pipes, and at a velocity so great that the sweep is distinctly audible when much water is moving.

No water should accumulate in the separator, as it is the mission of the loop to remove it before it assembles into a liquid mass. It is here that the constant and vigorous action is of great practical utility, enabling the loop to act as a preventive of accumulation rather than a device for removing water after it has accumulated. The separator evidently must be of such form as to give the sweep towards and through the loop better opportunity to pick up the entrained water than is afforded the current sweeping toward the engine, pump or steam-using device. It is interesting to know that experiments on very large separators, as large as 5 feet by 12 feet, not discharging their

contents, show that if water be allowed to accumulate to a certain height, no further rise will occur, even though much moisture be present, in the steam, showing that the steam sweeping through carries the water with it, unless that already deposited be drained away. Thus a separator to properly perform its function of drying steam must be empty, while obviously to stop and hold primage, its volume must not be impaired by periodic accumulations. Thus "solid" water in bulk is to be avoided. Condensation and entrainment should be retained and returned as spray or films, while primage should be broken up into particles or masses small as possible and instantly returned before a cumulative effect causes damage. In performing these functions the steamloop offers a new process in the apparently humble mission it serves.

That separators alone do not generally separate all the moisture from the steam is quite clearly shown by the fact that a loop will often return more water when the throttle is closed and the engine is idle, since the separator must then stop all moisture and the loop return it. With engine running, some moisture will always be carried over, the amount of course depending upon the efficiency of separator. Experiments on this feature are difficult because of the interference due to varying amounts of condensation, entrainment and primage, on all of which the draft of steam may have more or less effect.

Limitations.—Generally speaking, the limits within which the steam-loop is applicable are very wide, for the principle applies quite as well to great as to small differences of pressure. Similarly, an enormous quantity of water may be handled quite as easily as a small amount. The action will continue reliably through long pipes, overhead or underground. Water may be lifted from levels far below the boilers. The use to which the steam may be applied after the loop and separator have dried it, of course has no effect upon the loop system. Wherever steam is so used that it condenses rapidly, as in dryers, steam-heating systems, jackets, steam kettles, etc., the loop can be applied to the return of this water of condensation the same as from an

ordinary separator, and that, too, against any difference of pressure.

The above statements are made to illustrate how thoroughly and completely the loop can be applied to a wide range of conditions, but when we come down to the practical application, and say, how far is it expedient to apply it, the field contracts somewhat.

To take up the different limiting conditions, and comment upon their degree will show most clearly the practical range through which the loop may be made a commercial device.

Difference of Pressure.—The loop's application in this regard is limited most often by the head room for its erection. If the pressure in a separator, dryer, or return from a steam-heating system be ten pounds below the boiler, and a loop about thirty feet high is necessary to make the return, it is evident that a difference of fifty pounds in pressure would require a loop about 150 feet high and the riser, drop-leg and large portion of the horizontal being well covered with non-conductor, such a loop would operate efficiently, but generally speaking, a line of small pipe of that height would seldom be convenient, inasmuch as it would require some peculiar structure to hold it, or possibly, might need to be erected on the side of a smokestack. In high city buildings such a loop may be practicable where convenient air shafts allow easy support, but in ordinary manufacturing plants it would seldom be constructed.

While speaking of difference of pressure, attention should be called to the fact that the absolute pressure is of no importance, as a loop will work quite as well under low pressure as high. Its construction and operation recognize only the difference of pressure. A special case occurs, however, where the difference of pressure is very large compared with the lowest pressure in a system. For instance, if a boiler carries twenty-five pounds of steam and at the end of a series of heating or drying coils the pressure is one pound, then with a loop about 100 feet high it would be evident that if the condensation in the horizontal were so performed as to even produce a perfect vacuum, the water column in

drop-leg would stand about eighty feet high, but it is doubtful whether the pressure of one pound at foot of riser plus the 14·7 pounds due to vacuum would be sufficient to force the contents of riser up 100 feet and into the horizontal. It certainly would not be sufficient if there were considerable quantity of water to be handled, thus causing a high specific gravity of riser contents. Such case, however, is so seldom met in practical loop application, that it scarcely need be considered a limiting condition.

Roughly speaking, differences of ten to fifteen pounds are the largest experienced in good practice, and the loop can generally be conveniently erected to operate against such differences, and where excessive discrepancies in pressure are observed, it is usually very desirable to make such changes as will diminish differences, they being usually due to faulty piping. While, therefore, excessive difference of pressure is practically a limiting condition to steamloop practice, it is not found to be an annoying interference.

Distance of Foot of Riser below Water Level of Boiler.—Since the system is placed in equilibrium by the drop-leg column, and this starts from the boiler, no account need be taken of the distance the riser may extend below water level in the boiler; that is, the engine, separator or drying apparatus may be anywhere below with no effect upon the loop's action except the additional work imposed. This would, however, be a limiting condition when the riser column became so long that the pressure at its lower end is insufficient to lift the total weight of mixture into the horizontal. For instance, if the loop is working under five pounds difference, from fifteen pounds to ten pounds (see *Fig. 3*), and if the riser is so charged with water that the specific gravity is say ·1, then when the horizontal further reduces the pressure to say six pounds, there would be a total forcing pressure up the riser of four pounds, which would support a water column eight feet high or a column one-tenth as heavy eighty feet high. The horizontal in this case would stand about thirty feet above boiler water, hence the foot of riser can extend about fifty feet below boiler. Similar figures for eighty pounds boiler pressure, with seventy-five

pounds at engine and drop of ten pounds in horizontal would allow riser to run about 150 feet below boiler level. The above considers only a static column, but if we allow for the sweeping action of the current ascending the riser, the allowable distance of riser foot below boiler will be much increased.

Distance of Foot of Riser above Boiler.— Evidently the foot of riser can be elevated to any required extent, but there will be a height depending upon difference of pressure at

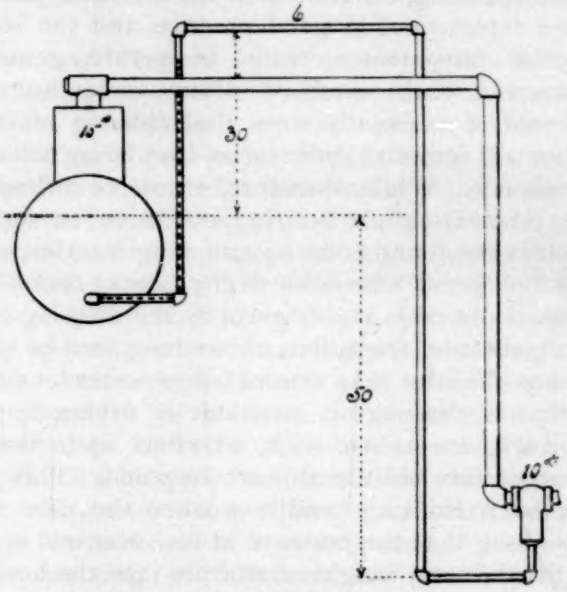


FIG. 3.

which the true functions of the loop may seem to become superfluous. Consider the case where the drain is just at a height above boiler water equal to the hydrostatic head due to the difference of pressure (see *Fig. 4*). Then, with no riser in the system, water collecting in the separator will run directly down a pipe, similar to a drop-leg, into the boiler. This water must collect in ordinary liquid state and exert gravity pressure before the column can move. The velocity of its exit depends wholly on the amount that has collected. Should the difference of pressure increase slightly, accumu-

lation is necessary to cause drainage. This is simply a gravity system. It has no current carrying the water swiftly from separator and over a hill to a point of safety on the top of a water column on the other side, which may rise and fall from time to time without affecting the flow from the point that is to be drained. A gravity system is a water pipe sufficiently high to overcome a difference of pressure. A steamloop may be called a gravity system from the boiler to the top of water column in drop-leg, but a

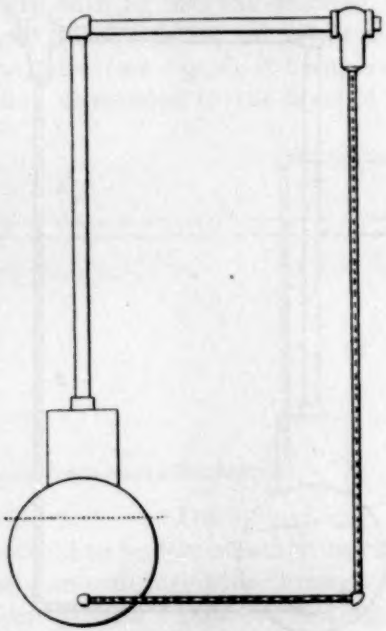


FIG. 4.

steamloop essentially is a device extending above a balancing column, with such functions as enable it to create a circulation that is not dependent upon gravity or the incoming water of condensation, but wholly upon the flow of steam from a higher to a lower pressure, and so arranging the mechanical device that this flow will carry water with it. Therefore, even though the source of condensation is far above the boiler, the principle of the loop is just as applicable to constantly and positively remove water as

though it were below and its virtues are just as essential. If the height above the boiler is far in excess of the head due to maximum difference of pressure, the loop may take the form of a straight pipe, the first section of which may be considered a riser, the second a horizontal and the last or lowest a drop-leg (see *Fig. 5*). Such a pipe would be a limiting case of steamloop, its shape distorted, but its functions retained. The true action occurring in such a pipe has heretofore been imperfectly understood. Indeed, it is

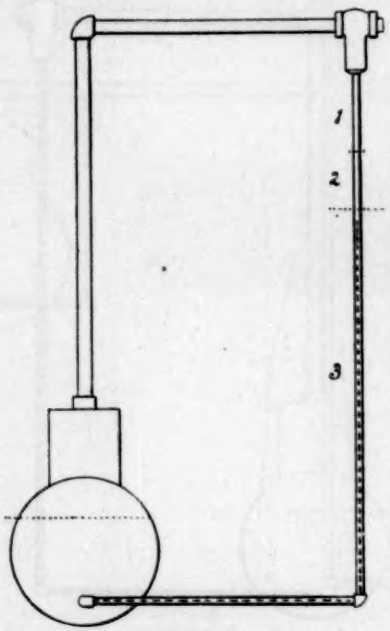


FIG. 5.

quite probable that no one has considered it deserving of especial consideration until the steamloop was devised and explained.

Distance.—It might not be unreasonable to suppose that the steamloop's application would be quickly limited by the distance of the separator from the boiler, and that the length would introduce complexity or uncertainty of action. Extreme length might impair its action, although even this is questionable, but at the greatest distances thus far met, of

800 to 1,000 feet, no adverse conditions have been encountered. Great distances cause proportional drop of pressure within the loop itself, requiring proper allowances to be made in height of drop-leg. The temperature of return is also affected, due to the cooling effect of long pipes, and can be largely controlled by proper covering. The drop of pressure mentioned above, due to the length of loop, occurs only when a horizontal is extremely long, hence is rarely met in practice. In extensive systems the horizontal is made only the functional length, turning into the drop-leg, which descends, and at a proper level can be led away through piping of any form to the boiler (see *Fig. 6*), it being evident that when the drop-leg has descended to the level of boiler water, all

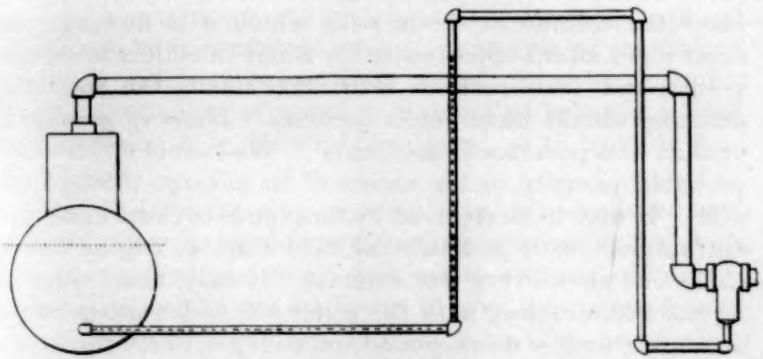


FIG. 6.

further connection to boiler, whether one foot or 1,000 feet, becomes merely an ordinary pipe through which water will seek its level as through a pipe connecting two barrels into one of which water is poured. Thus, distance will be noted to impose no practical limit on the loop's application, for it causes no change in the loop itself.

Quantity of Water Returned—This depends upon the amount of moisture entrained in the steam, the condensation in the pipe and the boiler primage. Usually there is little attempt to classify water appearing in a steam system according to the causes of its existence. Its presence is simply regretted until harm results, and then it may be less considerately mentioned. If water is only to be drained out,

as by drips, etc., when it accumulates in considerable volume, it matters little how it is formed, but when we come to handle it systematically, continuously, and with certainty, we must consider the source of the product on which we operate. *Water of entrainment* causes steam to be known as wet, and is usually constant in quantity. It may be separated from the steam in several ways, means for this being provided in the various forms of separators, and when so separated must be led away from the main steam current so continuously that it cannot again be picked up. *Water of condensation* is supposed to flow along the bottom or be swept along the sides of pipes, and is also usually constant in quantity. It requires no true separation, but must be led into a chamber of suitable size in such a manner as to readily leave the column of steam with which it is flowing. As most pipes afford opportunity for water to collect at certain points, and then rush on with the steam, the receiving chamber should have ample capacity. *Water of primage* is erratic. Its presence is accidental. We cannot estimate its probable quantity, or the nature of its passage through the pipe. It may be so received by the pipe as to cause excessive entrainment, as is probably the case when an engine labors, clicks and shows very wet exhaust. It may come over as liquid, and mingling with the water of condensation, cause an engine to slow down, pound and collapse, or spurt streams of liquid water from the exhaust. It may be constant or intermittent, and is always dangerous. It must be led away from the steam current, and received in a chamber whose capacity is considerably larger than that required for water of entrainment or condensation.

A separator must, therefore, provide ample facilities for separating the particles of entrained water from the steam and ample capacity for moderate primage. (Extreme primage is, of course, beyond control, and requires cure rather than care.) The loop must have capacity sufficient to continuously discharge the water collected from entrainment and condensation, and a maximum working effort, or single impulse, equal to taking in one liquid stream the contents of separator when filled up to a certain limit by sudden

primage. (If filled beyond this limit, we have a flood, whose control is not within the province of the loop or any other instrument, and must be left to Providence.)

This double service, for which the loop must be proportioned, involves the ratios existing between the volumes of the separator, riser and horizontal, for the horizontal must have a volume sufficient to take the contents of separator and drop-leg, and still have space left to be occupied by steam, whose condensation will maintain a continuous action while the water in the horizontal is draining through the drop-leg.

This action, while apparently intermittent, is not truly so. It is intermittent with reference to the extra charge of water carried, but the loop is simply exerting, in an emergency, its maximum ability, which, if constantly required, would not be inconsistent with its principle of continuity, although different proportions would then be desirable. Thus, the quantity of water to be handled becomes a limiting condition only when it is so great as to literally flood the system.

Devices for increasing capacity have been employed, such as cored risers, flattened risers, but experimentally they have not, as yet, produced such results as to warrant adoption.

When a loop is not sufficient to give the required capacity, it is not enlarged, but duplicated, and this process may be continued to any extent. When two or more loops are applied to one source, no difficulties are met in their operation, except some competition as to which will secure the business of returning the water. A little influence exerted, in the detail of pipe connections, enables them to form a trust, in which the spoils are equally divided with satisfaction to all concerned, even the public.

Another method, when primage is excessive, is to place a separator on the steam pipe near the boiler, with contents looped back directly, thus compelling the separator and loop at the end of the system to handle the water of condensation only. This method has been used with marked benefit (see *Fig.* 7).

Presence of Air.—To start the loop's action, the air must

be removed by blowing out with steam, hence it is proper to inquire whether air would not collect in the horizontal and gradually reduce the action and make it eventually become "air bound." There is certainly sufficient air in steam to produce this action, but in practice it does not. This may be attributed to the high velocity at which the loop current flows, carrying the air forcibly back to drop-leg where it enters the water and condensing steam, returning to the boiler. Only one instance is known where the loop has met difficulty of this nature, and that was due to boiler water containing much organic matter, the gases from which would collect in the loop. The remedy in such a special case is to apply a small stand pipe to the horizontal

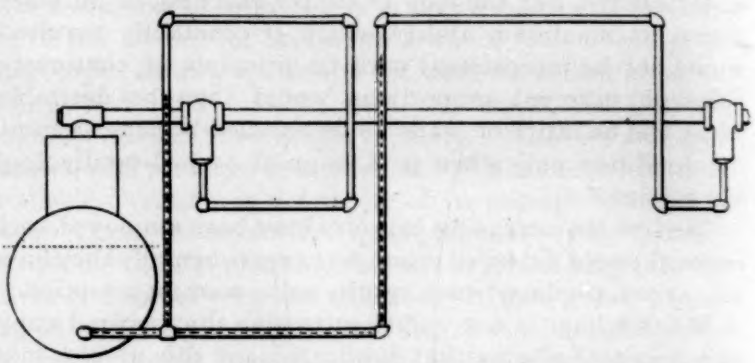


FIG. 7.

in which the air or gas can collect, cool, and, by difference of temperature, operate an air valve, allowing escape until steam rushes in and closes the valve.

To sum up the limitations, we find the loop action is practically independent of the distance that the source of supply is above or below boiler, and also independent of the length of return. It is not interrupted by the presence of air, and is capable of handling such quantities of water as usually exist in steam systems. It is practically limited by excessive differences of pressure and by abnormal quantities of water.

Constructive Features.—When a device is so simple as to consist of a run of one inch, or one and one-fourth-inch

pipe, it may seem unnecessary to endow it with so significant an attribute as constructive features, but there is a right and a wrong way to run even an inch pipe. When the mission is so special as that of the loop, there is only one right way and numerous wrong ways. To enter into this matter in detail would be too long for the present discourse, hence we will consider only a few points of especial interest and without extended explanation.

Practice has determined that one inch or one and one-fourth-inch pipe is most suitable for risers, while drop-legs generally take one and one-fourth inches to one and one-half inches, seldom two inches, and never larger. Horizontals commonly are two inches, but, as the volume is material, they may be four inches or even six inches if very short; with reference to them, however, no rule can be considered generally applicable, for the volume of the separator may figure as a large or small element, according to the necessity for handling primage, while the ratio of superficial area to volume is important in giving proper strength to the circulation and determining the economy with which it shall operate.

All horizontal portions of the loop should pitch downward with the current. It may be well to remark here that the terms "riser," "horizontal" and "drop-leg' do not necessarily imply that risers are always vertical, horizontals always horizontal, or that drop-legs follow a plumb line. A riser is the entire run of pipe from separator to highest point of the loop, and may have horizontal portions. The horizontal is the summit level of pipe in which condensation produces the loop's action, and though usually horizontal, is not necessarily so. The drop-leg is frequently inclined, and may have almost any form.

The riser should be provided with a stop valve and the drop-leg with stop, check and blow-off valves. The check must have approximately equal areas on both sides, or when shut it will be much out of balance and become a serious obstacle to the returning water. The return must be made with an independent connection, for if connected to feed pipe, the pulsations caused by pump would affect the drop-

leg, while if connected through blow-off, especially if it be in a mud drum, the loop may disturb the sediment.

The riser and drop-leg should invariably be covered with good non-conductor to prevent loss of heat, while the horizontal should be covered wholly or in part, according to its length and size, and the necessary condensation.

Combinations of Several Loops.—Thus far we have considered the loop as an isolated device, returning the water from one separator attached to one steam pipe. The application may, however, be extended throughout a large steam system; in fact, it is one of the important features that the entire drainage of a large system of steam pipes can be made with a system of loops forming a network of returns back to the boiler, affording ample opportunity to clear the system of water and with no chance for waste by escaping steam through faulty or open drips. It matters not whether the pipe be overhead or underground. Loops can be attached directly to the bottom of steam pipes at low spots and separators may be used at each steam outlet where engines, pumps, dryers or other apparatus operate. When the system thus extends, it is pertinent to examine to what extent the loops may be combined to simplifying piping. On first thought it may be proposed to simply run all the drains to one point, and there attach a loop of sufficient size, but the impropriety of this is at once apparent, since such loop would be liable to receive nothing but water and would be promptly flooded. Each loop is proportioned and erected with reference to specific difference of pressure and a given quantity of water, hence if the drainage of several separators into one loop is considered at all, these separators must be attached to pipes bearing such relation to each other that they will all be subject to equal differences of pressure. Consider a large and small engine side by side, the large engine having constant load while on the small one the load varies widely. If these separators be drained into one loop, there would be opportunity for a sudden drop of pressure near small engine, caused by sudden increase of load, bringing the contents of the large separator over to and possibly through the small engine. This could be

obviated by introduction of check valves, but they are undesirable, and the loop system is so capable of being made to work properly on its own intrinsic principles that it would seem to be bad engineering to create a set of conditions in which the loop needed auxiliary devices to counteract effects which should not be present. Similarly, when large quantities of water are received by one drain or separator, its presence might so overload the loop as to render it inoperative with respect to the several other connected sources. Therefore, one loop should never be connected to several sources of supply, except in rare cases where the different sources may be considered only as subdivision of one source; that is, when they are all subject to the same influ-

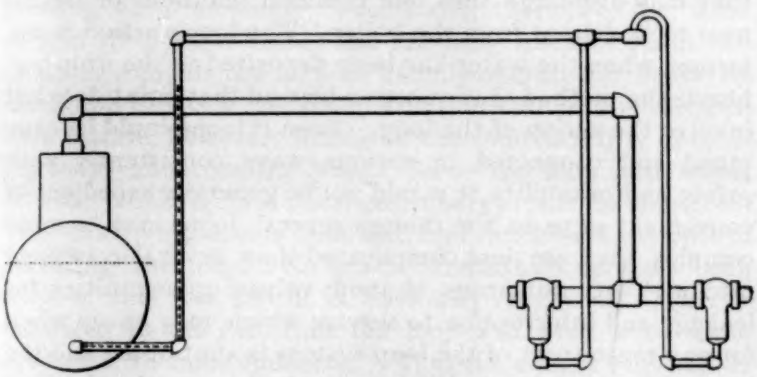


FIG. 8.

encing conditions. Each source of supply should therefore have its own riser. If, now, several risers from various sources enter one horizontal, we would afford opportunity for the loop's fundamental principles to be violated. The circulation must be uninterrupted and end at the point of lowest pressure. There cannot be more than one point of lowest pressure, hence with two or more risers connected into one horizontal, an accidental drop in pressure in any riser or pipe to which it is connected would short-circuit the loop from one riser to another (see *Fig. 8*). Therefore, each riser must have its own horizontal. We then consider whether several horizontals can enter one drop-leg. The balancing water column whose height is due to difference

of pressure must rise and fall with the variation of pressure. Hence, one loop may have a different height of balancing water column from another, even though they enter the same boiler. At the origin of one loop pressure may be increasing, while at another it is decreasing, calling for corresponding fall and rise of drop-leg columns, and if these loops were both attempting to utilize the same drop-leg we should have the anomalous condition that a column of water is required to rise and fall at the same time, and hence two or more horizontals should not enter one drop-leg. The numerous drop-legs in a given system do not, however, require independent connection to the boiler, for they may discharge into one common manifold or header near to or distant from the boiler. The loop's action is performed when the water has been deposited in the drop-leg; hence, the method of connection beyond that point does not involve the action of the loop. Even if loops could be combined and connected in various ways consistently with safety and reliability, it would not be generally expedient or convenient so to do, for though several loops may be more complex, they are less complicated than several combined into one with numerous shut-off valves, opportunities for leakage and interruption to service which may ensue when for any reason part of the loop system is shut down and the balance in operation. With individual loops the shutting down of part of steam plant simply cuts out single loops without effect upon the others.

Efficiency.—This is a feature of the loop on which it would be interesting to develop thermodynamic equations, and compare the same with experimental results, but the investigations on thermodynamic lines involve numerous assumptions on which the doctors will not agree, while experimentally we labor under the disadvantage of having no general case, each experiment requiring to be conducted on a special case, which may have but little bearing on others. By this it is not implied that the quantitative values of loop action are not or will not be determined mathematically and experimentally, but rather to say that such determinations are not of much import or interest in the

present discussion. A statement of general features surrounding the efficiency may, however, be interesting. The loop may, like an engine, operate idly, doing no useful work, because there is no water to return, in which case it would be infinitely wasteful, since the efficiency is the ratio of the cost of operation to the work done. The operative cost being practically constant in any given loop, its efficiency must depend wholly upon the amount of water returned and the maximum efficiency will correspond to the largest quantity that it is capable of handling. This limit, however, would seldom be reached in practice, for if constantly doing its maximum work the loop would be in danger of flooding each moment. Practically, therefore, the wide margin allowed for safety and handling unusual quantities of water compels the loop to usually operate far below its maximum capacity rendering it to that extent less efficient. This feature, however, indicates the inadvisability of making loops unnecessarily large. Since the loop puts water into the boiler against resistance, energy must be expended. This energy represents heat lost, and this loss is the cost of operating the loop. No exact determinations have been made of this loss, but it is obviously very small. This is indicated by the fact that the loop will work even when covered with non-conductor. That the energy required is very small may be deduced as follows:

A 100 horse-power engine using thirty pounds of water per hourly horse-power requires 3,000 pounds per hour. If ten per cent. or 300 pounds be returned to boiler per hour, and difference of pressure between separator and boiler is ten pounds, then about $\frac{1}{275}$ of one horse-power is required to do the work.

In the above it will be noted that we have assumed a very large amount of water to be returned, and have assumed a large difference of pressure against which to return it. Consequently the above figures are more than generous, and it is doubtful whether half this amount of power, or say $\frac{1}{500}$ of one horse-power, is used in the average case. As the loop is required to exert so small a power, it is evident that its efficiency, as measured by our ordinary ideas of economy,

might be extremely poor, and yet its efficiency for the purpose for which it is used very high; that is, while its absolute efficiency considered as a pump with which to get water into a boiler might be low, the small amount of energy needed to perform so valuable a service as continuous return, may make it relatively an article of high efficiency.

Another way of looking at this is as follows: A one-inch pipe may be presumed to lose by radiation ·1 of the heat which would be lost from a ten-inch pipe. Now, suppose a ten-inch pipe is running from a boiler to an engine, and the loss by condensation is being wasted through a drip, to say nothing of the steam also lost. Now, the loop will add one-tenth to the condensation but to compensate for this it returns to the boiler the original ten-tenths condensation in the pipe, plus the one-tenth condensation in itself, at a temperature (say) of 250° to 300°, which is evidently desirable as compared with wasting the condensation of the ten-inch pipe.

An important factor in its practical efficiency is the improbability of leakage existing, while other methods involve great opportunities in this direction.

There are other interesting developments with relation to efficiency which cannot be explicitly stated because of insufficient experiment. They may be indicated as follows: In any system which involves carrying from the boiler steam at a given temperature and returning a part of same to said boiler, there must be some loss of heat. The amount will depend on the differences of temperature to which the steam or resulting water is subjected and the time. All systems contemplating the collection of water in volume, handling it through pipes in volume and pumping it back in volume, contemplate or involve the use of apparatus having itself a temperature widely different from that of live steam from said boiler. They also contemplate the moving of a solid body of water which can only be done at a very low rate of speed. Hence, any return made on such system is essentially a wasteful one. The steamloop makes a return through apparatus which is small in mass and kept at a comparatively high temperature, while the current through it is

extremely rapid, bearing in this respect no relation to any other device. Both difference of temperature and time are so reduced as to give evidence of superior efficiency. In fact, it may be said that the steamloop cannot be reasonably compared with any other form of return that has yet been made, as it stands alone in its principle and utility.

This whole question of efficiency is, however, one of curiosity rather than importance, for, as previously stated, the whole matter is relative. The mechanical features of the loop system in removing the objectional water from pipes, through a continuous closed circuit back to boiler takes precedent over any questions of efficiency, especially considering the very small quantities of power and their heat equivalents involved. Just as an edge several inches wide may be sharp for the bow of a steamship and an edge .01 of an inch broad dull for a razor, so may the expenditure of a few thousand heat units in a lightly loaded steamloop be economical for a loop when the same number of hundreds of units would be uneconomical for a compound condensing engine furnishing the same foot-pounds of work. Loops, therefore, are simply to be compared with each other, and in this field there is still ample room for further accumulation of data.

In practice, the data obtained from large numbers of loops in operation vary so widely on account of various conditions imposed, that it is probably impossible to obtain reliable information from which deductions can be made, except on special apparatus erected on a large scale for experimental purposes only. The useful work done by the loop cannot be measured solely by the amount of water returned to the boiler, for if this water is all condensation and primage, its removal produces less economic effect on an engine than if it were held in suspension in the steam. Thus it may be that the exertion of a small fraction of a horse-power by a loop may cause the steam which it dries to exert many times more power in its expansion in the engine cylinder. The loop to do this might be lightly loaded, so that its own efficiency becomes relatively poor, while near by there might be a loop, heavily charged with water of primage and con-

densation, whose efficiency is relatively high, yet whose economic effect upon an engine is of less value. Therefore, the thermodynamic efficiency of a given loop is not proportional to its value to the user, even though we make no account of its value as an assurance against accident by water.

Results.—The result of steamloop action may be briefly summed as follows:

(1) Saving the water of condensation, entrainment and primage.

(2) Saving the heat contained in said water.

(3) Saving the steam systems from water, thereby reducing liability to accident.

(4) Returning pure water to the boiler.

(5) Preserving uniform temperatures, thus obviating difficulties due to expansion and contraction.

(6) Prevention of direct loss which usually exists from open drains, drips, tanks, etc.

(7) Enabling engines to start promptly.

(8) Maintaining higher pressures at the end of long lines.

(9) Maintaining higher temperatures in jackets, dryers, etc.

(10) Increasing the efficiency when steam is used expansively.

All of the above are more or less evident from what has preceded, except the latter, and on this there is some honest difference of opinion. The earlier writers on thermodynamics make little mention of the effect of entrained moisture on the expansive properties of steam, but by common consent rather than any demonstration, they seem to agree that moisture produces an ill effect simply to the percentage amount of its presence. That is, five per cent. moisture will increase the water rate of an engine only five per cent. Experimental data on this point seem very meagre, the only recent work being that of Prof. James E. Denton, of Stevens Institute, on a comparatively small engine in which the result indicated was substantially as above. Prof. Denton, however, considers further experiment necessary to establish a con-

clusion. The opinion of other authorities on this question, including Dr. R. H. Thurston, of Cornell University, and Geo. H. Barrus, M.E., of Boston, agree that the presence of moisture in steam should, and doubtless does, cause an injurious effect in excess of its own percentage. Indications of this are observed in superheated steam where a given amount of heat expended in superheating produces more saving in the quantity used by the engine than the equivalent in added heat, and the same condition should extend to supersaturated steam. Definite knowledge of this is, however, yet to be obtained, but it seems to be the general concensus of opinion that moisture is detrimental to an extent about double its percentage; that is, five per cent. moisture in steam, effects the efficiency of an engine about ten per cent. Further development and opportunity for experiment with the steamloop system will aid in determining this.

Other Applications.—Throughout this discourse the description has been confined almost wholly to the application of the loop to the one case where moisture is to be removed before steam passes to an engine or pump. It is thought best to keep this one case clearly in mind, for the loop thoroughly understood on this basis may be easily conceived to serve similar purpose in any other connection. Where live steam is used for drying purposes, the loop may be attached directly to the return, this maintaining a powerful circulation through the heating coils and ridding the system from the condensation which is the natural product of the heating or drying process. In this service, however, the loop has opened up a new feature, that of drying the steam before it enters such heaters, and it is found to yield very beneficial results, by keeping up temperatures and pressures (see *Fig. 9*). Similarly, steam kettles, jackets of steam-jacketed cylinders, and even steam-heating apparatus can be handled with ease and efficiency. Much apparatus of this nature, however, is throttled down to a degree that seriously interferes with loop application, and in ordinary steam heating the opportunity is exceptionally good for large air leakage, which would be deleterious. It is, there

fore, not easy to concisely state to just what purposes the loop may be practically applied, but it is safe to say that it is desirable on any live-steam pipe or any high-pressure or unthrottled dryer, heater or jacket.

The development of the loop system has not stopped with the simple applications here recorded, but has been carried into fields of wider application. Perhaps no device has appeared during late years on which more thought and careful consideration has been bestowed. Its possibilities have been explored and its limitations studied to such an extent that the whole evening would scarcely suffice to enumerate

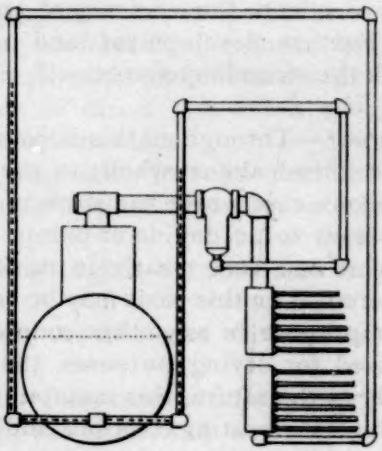


FIG. 9.

features of interest that have not been included in this lecture.

Thus, the steamloop becomes a device of much broader proportions than appears from first inspection of its operation or the rudimentary understanding of its principles. It may seem strange that its application has not before been developed, but that is doubtless due to want of clear comprehension of its somewhat peculiar functions and their utility, for those who are connected with steam engineering must in many ways and under various circumstances have had opportunity to observe the phenomena which concern

its action. We are, therefore, entitled to respect quite highly this useful combination, whose worth can never be accurately estimated, since its chief service is to prevent certain losses and accidents of variable character, and whose extent is the only measure of the loop's value.

THE PATH OF A PROJECTILE.

By F. GILMAN, Lowell, Mass.

The object of the present article is to show how well the equation of a projectiles path, developed on the hypothesis that the resistance varies directly as the velocity, conforms to the experimental results of the Chief of Ordnance, U.S.A.

I shall also submit the equation to the same test when developed on the hypothesis that the resistance varies as the square of the velocity, although in this case the comparison will necessarily be restricted to experiments in which the angle of elevation was very small. As the equation of the trajectory deduced on the assumption of a resistance proportional to the velocity, is not generally given in treatises on mechanics, we will briefly show how it is derived.

Let the resistance be expressed by $c\ v$, in which v is the velocity, and c a constant that expresses the resistance for a unit of velocity. The origin of coördinates will be taken at the point from which the projectile is thrown, x being the horizontal and y the vertical axis.

Let t denote the time, and g the acceleration of gravity. Then, according to the fundamental equations of mechanics:

$$\frac{d^2x}{dt^2} = -c\frac{dx}{dt} \tag{1}$$

$$\frac{d^2y}{dt^2} = -g - c\frac{dy}{dt} \tag{2}$$

Let

$$b = \frac{dx}{dt}$$

and substitute this in (1).

It becomes

$$\frac{db}{dt} = -cb$$

$$dt = -\frac{db}{cb} \tag{3}$$

and integrating,

$$t = -\frac{1}{c} \log b + C$$

in which C is the constant of integration.

In order to determine its value, let V denote the initial velocity of the projectile, and a the initial value of the angle which its trajectory makes with the axis of x. Then, since the time is estimated from the beginning of the motion.

$$o = -\frac{1}{c} \log (V \cos a) + C$$

$$C = \frac{1}{c} \log (V \cos a)$$

whence,

$$t = \frac{1}{c} \log \left(\frac{V \cos a}{b}\right) \tag{4}$$

Multiplying equation (3) by b, we have,

$$bdt = dx = -\frac{db}{c}$$

and integrating with due reference to the value of the constant,

$$x = \frac{V \cos a - b}{c}$$

whence,

$$b = V \cos a - cx$$

and substituting this value of b in the above expression for t, we obtain,

$$t = \frac{1}{c} \log \left(\frac{V \cos a}{V \cos a - cx}\right) \tag{5}$$

This equation gives the time required for the projectile to reach any point in its path whose horizontal distance

from the origin is equal to x. In order to integrate equation (2), substitute b for

$$\frac{dy}{dt}$$

and proceed exactly in the same manner as for equation (1). We find,

$$t = \frac{1}{c} log \left(\frac{g + c\ V sin\ a}{g + cb} \right). \tag{6}$$

Equations (4) and (6) give the time required for the projectile to reach any point in its path.

Equating them and restoring the values of b:

$$log \left(\frac{V cos\ a}{\frac{dx}{dt}} \right) = log \left(\frac{g + c\ V sin\ a}{g + c \frac{dy}{dt}} \right)$$

Passing from logarithms to quantities, taking the reciprocals and multiplying through by dt,

$$\frac{dx}{V cos\ a} = \frac{gdt + cdy}{g + c\ V sin\ a}$$

Integrating, substituting for t its value from (5), and solving with respect to y,

$$y = \frac{g + c\ V sin\ a}{c\ V cos\ a}\ x - \frac{g}{c^2} log \left(\frac{V cos\ a}{V cos\ a - cx} \right) \tag{7}$$

which is the equation of the trajectory.

An analysis of this equation shows that a perpendicular to the axis x at a distance from the origin equal to

$$\frac{V cos\ a}{c}$$

will be an asymptote to the curve.

Differentiating the equation, and finding the value of x from

$$\frac{dy}{dx} = o$$

we obtain

$$x = \frac{V cos\ a}{c} \left(\frac{c\ V sin\ a}{g + c\ V sin\ a} \right) = \frac{V^2 sin\ a\ cos\ a}{g + c\ V sin\ a}$$

which gives the horizontal distance from the origin at which the projectile attains its greatest elevation. This value of x substituted in equation (5) gives

$$t' = \frac{1}{c} \log \left(\frac{c\, V \sin a + g}{g} \right)$$

which gives the time in which the projectile will reach its greatest height. The expression for this height is

$$y = \frac{1}{c} (V \sin a - g t')$$

From equation (7) we deduce the following formula for the range

$$R = \frac{g\, V \cos a\, t}{g + c\, V \sin a}$$

The value of c depends on the initial velocity, the nature of the projectile, and the angle of elevation at which it is thrown. For each class of projectiles, and for each initial velocity, taken at intervals of from 50 to 100 feet per second, there will be a certain curve whose abscissas will represent the angles of elevation, and its ordinates the corresponding values of c. *Fig. 1* shows such a curve, which gives the values of c in the experiments made by the Chief of Ordnance on the firing of two guns, and recorded in his report for 1885, pp. 66, 495. One of the guns was a fifteen-inch Rodman smooth bore. The projectile was round iron solid shot, and its weight 455 pounds. The initial velocity was 1,678 f. s.

The other gun was a 3·2 inch breech-loading rifle. The projectile was a solid band experimental shell, and its weight thirteen pounds. The initial velocity was 1,602 f. s. The values of c, plotted from these two sets of experiments, gave two curves, which ran so nearly together, that it was thought best to combine them into one mean curve, providing that the computed results would agree with the experiments, as well as the experiments agree with themselves.

The following table gives a synopsis of the results:

15-INCH RODMAN GUN.

Angle of Elevation.	Value of c taken from Diagram, *Fig. 1.*	Range		Time of Flight	
		By Experiment.	By Formula.	By Experiment.	By Formula.
		feet.	*feet.*	*seconds.*	*seconds.*
2°	0·0673	5,300	5,230	4·0	3·5
5°	0·1112	8,857	8,810	8·1	7·9
10°	0·0890	13,696	13,636	13·9	14·9
15°	0·0748	16,651	17,319	19·1	21·5
20°	0·0660	19,285	20,043	23·8	27·7
23°	0·0622	21,243	21,968	26·4	31·2

3·2-INCH BREECH-LOADING RIFLE.

Angle of Elevation.	Value of c taken from Diagram, *Fig. 1.*	Range		Time of Flight	
		By Experiment.	By Formula.	By Experiment.	By Formula.
		feet.	*feet.*	*seconds.*	*seconds.*
2°	0·0673	4,755	4,797	4·0	3·3
4°	0·1148	7,093	7,096	6·7	6·2
6°	0·1065	9,109	9,215	9·0	9·0
8°	0·0974	10,907	11,066	11·5	11·7
10°	0·0890	12,450	12,770	13·7	14·3
12°	0·0818	14,472	14,354	15·7	16·9
14°	0·0770	15,810	15,658	17·9	19·4
16°	0·0725	17,070	16,890	20	21·9
18°	0·0690	18,291	17,944	22	24·3
20°	0·0660	19,437	18,870	24	26·6

The fourth and fifth of the Rodman experiments show the greatest discrepancies, amounting to about four per cent. of the range. It will be found, however, on examining the separate experiments of which these are the mean, that they show differences among themselves of from seven to eleven per cent. The initial velocity was measured only in the first few experiments, and it was assumed to be the same in all the others, as the weight and kind of powder was the same. But it is probable that this assumption is

not strictly correct and it would have been desirable to have had a measurement of the initial velocity at each experiment, as it seems likely that the differences in the ranges corresponding to the same angle of elevation are principally due to differences in the initial velocities. The c curve given in *Fig. 1* of accompanying plate, is peculiar in that it shows such a great variation in the value of c for angles of elevation between 2° and 4°.

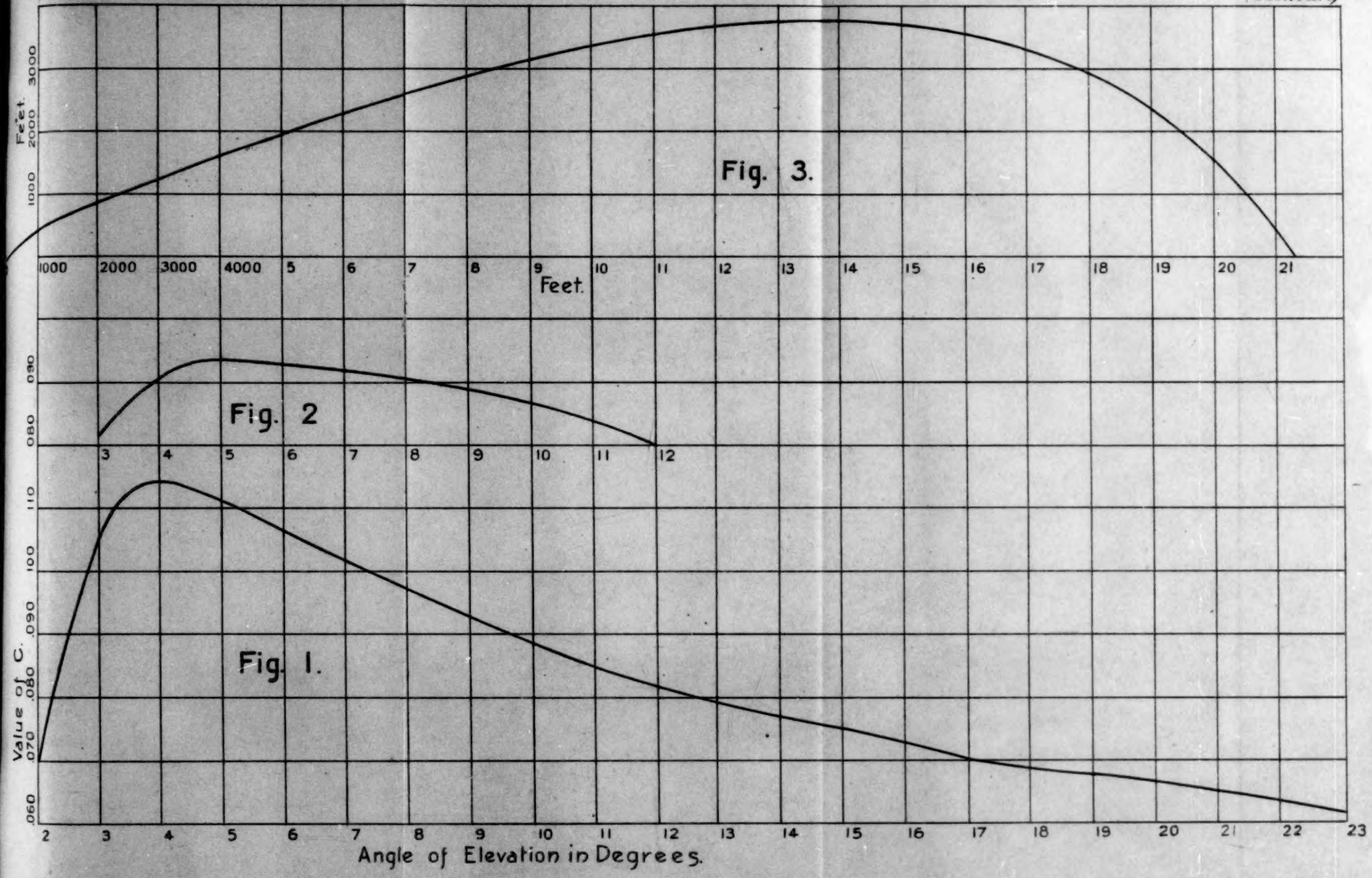
Beyond 5°, as the angle of elevation increases, the value of c gradually diminishes, and the curve is very regular.

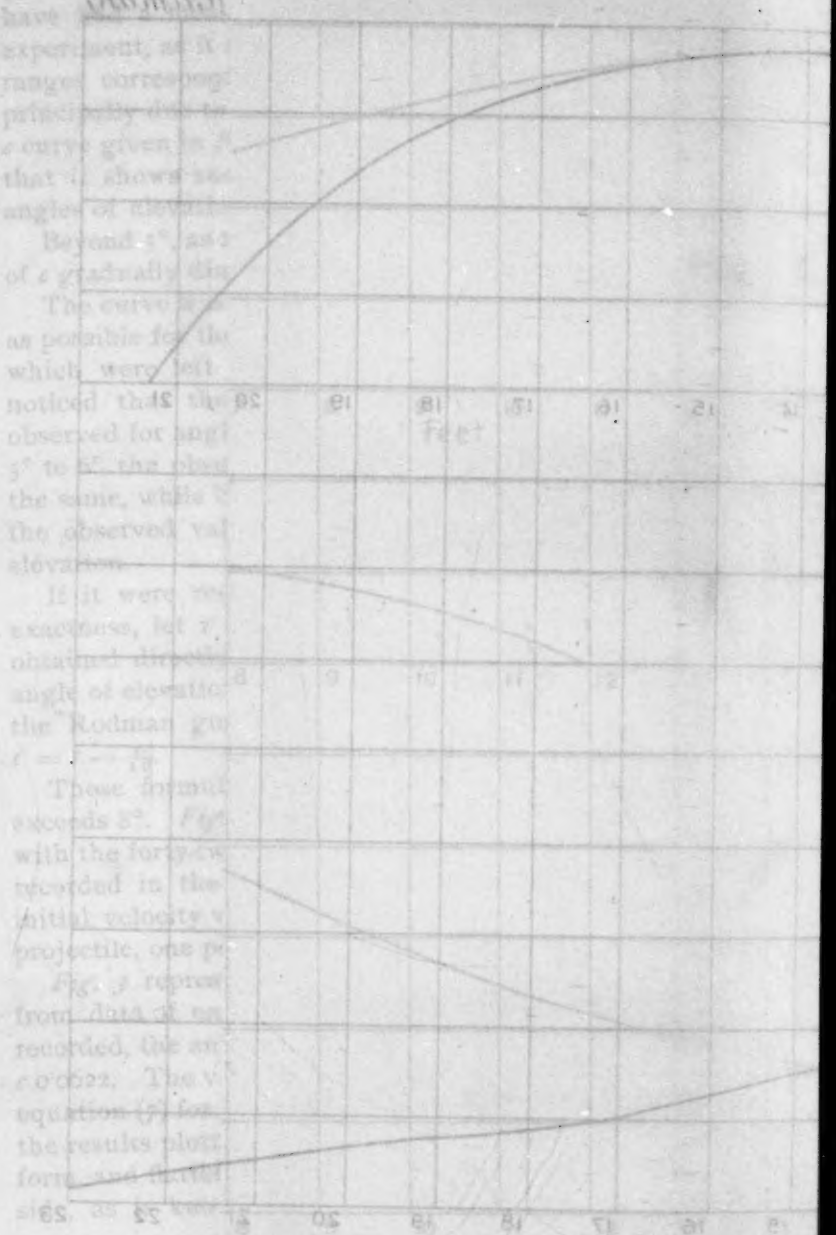
The curve was constructed so as to give as correct values as possible for the ranges, and without regard to the times, which were left to come out as they would. It will be noticed that the computed times are smaller than the observed for angles of elevation of from 2° to 5°, that from 5° to 6° the observed and computed times are practically the same, while beyond 6° the excess of the computed over the observed values gradually increases with the angle of elevation.

If it were required to compute the time with greater exactness, let t' denote the corrected value, t the value obtained directly from formula (5), and denoting by n the angle of elevation (expressed as seconds), we will have for the Rodman gun, $t' = t - (\frac{n}{10})^2$ and for the 3·2-inch rifle, $t' = t - \frac{n}{10}$.

These formulas will apply when the angle of elevation exceeds 8°. *Fig. 2* shows the c curve for experiments made with the forty-two millimetre Hotchkiss mountain gun, and recorded in the Ordnance Report for 1885, p. 88. The initial velocity was about 1,300 f. s., and the weight of the projectile, one pound fifteen ounces.

Fig. 3 represents the path of a projectile as deduced from data of one of the Rodman gun experiments above recorded, the angle of elevation being 23°, and the value of c 0·0622. The values of the ordinates were calculated from equation (7) for every 500 feet of horizontal distance, and the results plotted, give the curve. It is unsymmetrical in form, and flatter on the ascending than on the descending side, as is known by observation to be the case, in the


Feet.
1000
2000
3000
Fig. 3.
1000 2000 3000 4000 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21
Feet.
Fig. 2
3 4 5 6 7 8 9 10 11 12
Value of C.
.060 .070 .080 .090 100 110
Fig. 1.
2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23
Angle of Elevation in Degrees.



actual path of a projectile. The horizontal distance from the origin at which the projectile attains its greatest elevation is, in this example, about two-thirds of the range.

In order to show how well these ordnance experiments sustain the hypothesis of resistance proportional to the square of the velocity, we will take the equations that apply to this case, and which are generally given in treatises on mechanics.

$$t = \frac{1}{c\,V\cos a}\,(e^{cr} - 1)$$

$$8\,c^2\,h\cos^2 a\,r\tan a = e^{2cr} - 2\,cr - 1$$

In these equations t represents the time of flight, a the angle of elevation, v the initial velocity, r the range, h the height due to the initial velocity, and c the constant of resistance. Since these equations can be applied only to cases in which the angle of elevation is very small, we will select for comparison the first two of the Rodman gun experiments.

We find that the value of c, which is obtained by substituting for r the correct range, is in the first experiment 0·0000386, and in the second 0·0000810. The corresponding realness of the times are 3·5 seconds and 7·7 seconds.

We see then that for angles of elevation between 2° and 5° c is even more variable than was found to be the case on the hypothesis of a resistance proportional to the velocity, and as the constancy of the value of c (at least for small angles of elevation), would seem to be a good test of the accuracy of the hypothesis on which it is based, it follows that the assumption of a resistance proportional to the square of the velocity is no better sustained by these experiments, than was the assumption of resistance directly as the velocity. The values of c for the first two experiments with the 3·2 inch breech-loading rifle, are 0·0000486 and 0·0000855, and the corresponding values of the times 3·3 and 6·1.

It appears that the resistance of the air can hardly be expressed in terms of any single power of the velocity, but

I think it can be expressed quite accurately by the following formula:

$$R = a + bv + cv^2$$

in which R is the resistance, v the velocity, and a, b and c three constants to be determined by experiment. I have applied this formula to the experiments of Hutton on air resistance, and after determining the values of the constants by least squares, found that it satisfied those experiments within the limits of probable errors of observation. The range of the velocities was from 950 to 2,050 feet per second.

The greatest difference between the computed and experimental resistance was about three per cent., and the most of them were much smaller.

But, although this formula probably represents the law of resistance, yet to attempt to make it the basis of a mathematical deduction for finding the equation of a projectile's path would be a hopeless problem, from its complexity. It seems more practicable to find the elements of the trajectory by the first method shown above; especially since the results of that method are as accurate as those arrived at on the generally accepted hypothesis of resistance proportional to the square of the velocity.

THE GALVESTON HARBOR PROBLEM.

BY LEWIS M. HAUPT, A.M., C.E.
Professor of Civil Engineering, University of Pennsylvania.

The importance of securing a deep water harbor on the west coast of the Gulf of Mexico in developing the commercial, industrial and agricultural resources of the United States, and especially of the Trans-Mississippi region, can hardly be over-estimated.

The many elements affecting the problem in its commercial, and to some extent in its political, relations, have been ably stated in the recent paper on "Southwestern Commerce and Gulf Harbor," by Senator W. P. Frye, who presents the subject from the standpoint of the statesman, but it is quite as important that the question should be considered from the standpoint of the engineer, as it is upon this phase that the success of the project must depend.

The desirability of such an outlet for the products of the 1,000,000 square miles of tributary territory does not admit of doubt. The practicability of securing it has led to several animated discussions in the engineering world as well as in Congress, and while definite and positive conclusions have been reached by individuals, the great majority of legislators regard the problem as a huge "experiment," and are willing to defer formulating an opinion until results are obtained.

In engineering, as in legal practice, precedents are invaluable and failures teach more than successes, as they often determine the limits within which a given method may be applied with success. It is the purpose of this argument to apply this test to the locality in question, considering it first, *per se*, in the light of its own history, and second by comparison with existing conditions and results elsewhere so far as they may be applicable.

The peculiar difficulties and great importance of this question have given zest to the discussion and have

attracted the highest grade of talent to its consideration, hence its history will be found bristling with interest even to the unprofessional reader.

When, in 1869, the writer was on duty as Engineer Officer of the Fifth Military District (Texas), it was often impossible for the Morgan Line steamers, built expressly for the Gulf trade, to cross the bar at Galveston and other ports of Texas. Passengers, cattle, baggage and freight were frequently lightered ashore at great risk of being dumped into the water, or drenched by the breakers on the bar, and to this day, at many of these ports, no improvement has been effected in this tedious, dangerous and expensive mode of effecting a landing. Then, the outer bar at Galveston was three miles from Fort Point and had a channel depth across it varying from twelve to twelve and one-half feet, and vessels frequently had to "lay to" outside the bar for days awaiting a favorable wind and tide before undertaking an entrance. Having passed this barrier and rounded Fort Point a contracted channel was entered, which constitutes the harbor proper. This channel lies between Galveston and Pelican Islands, and is separated from the outer roadstead by a recent formation known as the inner bar.

In the report of Lieut. Stanton, made in 1868, it is said: "The harbor proper, obstructed by this bar, is 6,000 yards long and only 250 yards wide, measured within the 12-feet curves, affording to ships of 12 feet draught very scant space for turning about." Again, in the report of the Board of Engineers of January 21, 1886, occurs this statement: "On the north side of the island in front of the city of Galveston depths of 35 feet are found in a narrow channel. This area of 270 acres forms Galveston Harbor." * * "The main entrance to the bay * * has a present width of 8,200 feet." The depth here, in the "gorge," is 40 feet. "The interior of the bay, aside from the main entrance, has depths which do not exceed 11 feet at mean low water."

As great stress has been laid upon the capacity of the harbor for deep draught vessels, these facts assume con-

siderable importance, in view of the plans proposed for improvement, and particularly with reference to the exaggerated impressions which prevail, due, doubtless to a superficial inspection of the charts of Galveston Bay, with its 451 square miles of area.

One of the recent official reports * on this subject, guardedly states: "The entire area, inside the outer bar, containing a least depth of 30 feet, is 460 acres, and there are 1,300 acres with a least depth of 24 feet, but these areas can scarcely with safety be referred to as Galveston Harbor without explanation, because over 500 acres of the 1,300, with a least depth of 24 feet, lie outside the gorge."

In fact, these 1,300 acres are embraced in the deep pocket, situated between the two sandy barriers separating the bay from the Gulf, and the outer and inner bars. They constitute virtually an outer, open roadstead, separated from the deep waters of the Gulf by the outer bar with its present maximum depth of 13 feet, at only one point on its crest and exposed to the full force of the Gulf storms—a more difficult trap to escape from than that of Apia and one possessing very few of the requirements of the harbor, even of refuge. The "White Squadron" on its recent visit to Galveston, was unable to enter this "harbor," not deeming it safe to venture nearer than about ten miles. These physical features must be given due weight in any comparison of sites for a terminal for the great Northwest. The problem is not local, but national, and the law of transportation which most affects the site, viz: that the water carriage should be continued as far inland and the land transit be made as short and easy as possible, should not be violated.

The inner bar is of comparatively recent formation. The "oldest inhabitant" remembers when it had no existence, and where there were 30 feet of water, in 1841, the depth had shoaled to 12 feet in 1853, due to material being carried in from the outer shore of Galveston Island.

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* *Ex. Doc. No. 56*, House of Rep., 51st Congress, 1st Session, dated Dec., 1889.

Up to September, 1854, the shoaling continued until the depth was but 10 feet, when a violent storm cut away its crest making a channel across it 15 feet deep. This depth remained very constant for eleven years, and in 1865, it was reported to be 14½ feet, but in 1867 the Coast Survey found only 9½ feet on the bar. *

The estimate submitted in 1868, for improving its depth so as to equal that of the outer bar, viz: 12 feet, was $1,300,000.

Such were the physical conditions before any material improvements were started, but the necessity for increasing the depth on the inner bar and the dilatory measures of the Government compelled the city of Galveston to undertake the work of constructing a pile-jetty over a mile in length in 1869, resulting in an increased depth of two feet.

This jetty was destroyed in part by the storms of 1875, aided by the ship worm (*Teredo navalis*). The first Government appropriation was made July 11, 1870, $25,000, followed by $20,000 the next year, and the engineer officer in charge was directed to submit a project for excavating and preserving a channel 80 feet wide and 12 feet deep across the inner bar "by the ordinary method of dredging."

After the lapse of over a year consumed in surveys and futile efforts to let the work by contract and in building dredges and scows, the officer in charge reported that "the work is not susceptible of permanent completion."† The year following he reported on July 20, 1872, that the dredge boat, steam tug and two scows, procured with the previous appropriations, "were not needed or worked during the year, due to the good effect produced by the jetty constructed by the City of Galveston," which caused a channel on the inner bar equal in depth to that on the outer bar. Under the circumstances the plant was held in readiness to work in case the jetty should be destroyed.‡

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* *Report Ch. of Eng'rs*, 1868, p. 509.

† *Report Ch. of Eng'rs*, 1871, p. 518.

‡ *Report Ch. of Eng'rs*, 1872, and was loaned to citizens to improve their docks.

The next appropriation of $31,000, June 10, 1872, was to be expended in surveys and maintaining the Galveston jetty. The year 1873 was consumed in surveys and further considerations, resulting in a project for securing an 18-foot channel over the outer bar by means of two jetties composed of "gabions filled with sand," at an estimated cost of $1,259,446.43. This project was submitted to the Board of Engineers at New York, who reported favorably in February, 1874, submitting a revised estimate, amounting to $1,759,401.85. The plan provided *two paralleled and submerged* jetties extending outward into the Gulf over *two and one-fourth miles apart*, and having a total length of 37,000 feet or over seven miles; an average distance of three and one-half miles from shore. For various reasons work on the jetty was not commenced until April 14, 1877, and from that date to January 1, 1880, the North jetty had been extended to sea nearly two miles. An examination made during that year revealed the fact that "the gabionade (North jetty) had essentially gone down below the original bottom, consequently it could not serve to direct or confine the currents."

Thus a decade passed in beginning the series of experiments to improve the outer bar. In the meantime, attention being thus diverted from the inner bar, it was left to the natural forces to maintain, and in September, 1875, a violent storm occurred which cut a new channel 12 feet deep through it at a point where the depth was only 7 feet, without reducing that of the old channel. This natural cut continued to improve, and by the following June had deepened to 17 feet. This gale of September, 1875, having destroyed the greater portion of the plant and material to the value of $50,000, several months were consumed in preparing to renew operations, by which time the appropriation was exhausted and that for the year 1876–77 being withheld, work was totally suspended until September, 1876, when a portion of it was released. Work on the inner bar was "completed" during that winter and then suspended until an equal depth on the outer bar should be obtained. The Fort Point gabionade having

been completed by June, 1877, it was observed that the deepening of the channel, started by the storm, still continued. The depths reported were in August, 1877, 16¾ feet; September, 1877, 18¼ feet; October, 1877, 19¼ feet; June, 1878, 20¼ feet. This increased result may be attributed to the interception by the jetty, of the sand formerly carried into the channel, rather than to any scour produced by contraction of the channel.

As to the work on the outer bar, it was found, as already stated, that the gabions, which were 6 feet high had sunk from 4 to 7 feet, and that, "contrary to expectations, they did not become covered with sand, but remained standing in a trench entirely uncovered,"* although submerged. These and other peculiarities led the Board of Engineers to reconsider their plans of 1879, and to abandon the gabionade in favor of brush and stone jetties, which it was affirmed would "undoubtedly succeed." "The theory upon which the Board relies is that of contraction of the outlet to force the outflow into a narrow channel directed upon the bar to effect its removal, and lessen, if not entirely prevent, for a long time to come, its reformation." †

Herein is where a fatal mistake was made in attempting to apply a theory which was inapplicable to the existing conditions. To produce scour by contraction the jetties must be so close as to co-act upon each other, that the trenches produced by their reaction may meet, and so prevent the formation of a middle ground. To secure this result at South Pass the distance between the jetties was contracted from 1,000 to 600 feet, and there the current is rapid and nearly continuous towards the Gulf. At Galveston it is intermittent and the ebb is dependent on the free admission of the flood to the inner bay. To place two jetties so close as to co-act and to scour by their reaction on the currents, would virtually have shut out the force available for scour and would have so obstructed the outlet to this bay that in great storms the city would have been endangered. In this

* *Report Ch. of Eng'rs*, 1879, p. 911.

† *Report Ch. of Eng'rs*, 1880, p. 1205.

dilemma, this board located the two jetties with an interval of over *two miles* between them, expecting by this plan "to force the outflow into a narrow channel," and to obtain a depth of at least 25 feet with a mean tide of only 14 inches. No precedent for such a plan exists in the history of the world, and yet the statement is made that after exhausting the literature of the subject "the application of this principle offers every guaranty of success."* "The suggestions as to details of construction are tentative only," and the Board recommended the trial of several methods of construction, among them that the line of "the South jetty should incline more to the North, so as to contract the opening between the two at their outer ends, but still remaining more than two miles apart;" and add "it has always been assumed that the Galveston jetties are to be submerged, but to what depth can only be determined by results produced during the progress of their construction. Should an average height of 5 feet above the sand bottom prove sufficient, it seems probable they can be constructed for $40 per lineal foot." † The total estimate for a depth of 25 feet across the bar was $1,825,813.

Under this project, as modified, work on the submerged brush-mattress jetties was commenced in July, 1880, near Fort Point, under the supervision of a new officer in charge, and continued as rapidly as the exigencies of the case would permit. Work on the North jetty was discontinued during the experiments on the other. By June 30, 1882, 20,777 feet of mattress work (nearly four miles) had been laid on the bottom and 10,130 feet of the second course was in place. These were ballasted by stone brought from a distance.

At this date, it was reported that "the effect of this work was not such as one unfamiliar with the physical phenomena of our locality would be led to expect." (The survey gave a depth on the bar of 11½ feet.) During the succeeding year, 1883, it was stated by the civil engineering assist-

* *Report Ch. of Eng'rs*, 1880, p. 1205.

† *Ibid.*, pp. 1229–30.

ant that "the gulfward movement of the bar, without a corresponding increase in depth upon its crest, would seem to indicate the importance of an early and rapid construction of the North jetty." Yet, although attention was thus early directed to its importance, up to this date nothing has been done on this part of the project, whereby the rapid advance of the bar might have been prevented and great expense saved.

The survey of 1884 "brings out prominently the fact of the advance of the outer bar gulfward, which effect was sought to be prevented by the rapid construction of the jetties." * * "Slight changes are noticed upon the bar, but no benefit to navigation has resulted yet."

Channels dredged on the bar are quickly filled up by the drift of sand from the North. "The North jetty is absolutely required, if it is expected to secure deep water."

This North jetty, as designed by the assistant civil engineer, and which was the most promising feature of that period, was a detached, concave structure rising above high water, and placed so as to protect the channel over the bar from the drifting sand and waves, which is the requirement of prime importance in this problem. It was to have been about three miles in length, but was never built, although approved and recommended by the officers in charge.

The appropriation bill having failed, the city of Galveston again came to the rescue with a gift of $100,000 to continue operations, and at this juncture Mr. Jas. B. Eads was induced to make a proposition to the Government to undertake the work on the basis of his offer made for the South Pass, of compensation only after results were obtained. To this end a bill was introduced into Congress, February, 1884,* authorizing him to undertake the securing of a 30-foot channel at a cost of $7,750,000, payable by instalments for each foot or more of gain in depth.

This bill was defeated by such plausible arguments as these, submitted through and by the Chief of Engineers: "Nine hundred and seventy-five thousand dollars has been

* Senate, No. 1652.

expended upon the construction of the South jetty, and the construction of the North jetty will cost $700,000. That the construction will give more than an additional two feet of depth in the channel connecting the harbor of Galveston with the Gulf is entirely certain." "The case can be plainly stated as follows: (The Government) with the expenditure of $700,000 and two seasons' work will obtain an increase of depth exceeding 2 feet and probably reaching 5 or 6 feet, while Mr. Eads and associates promise, after a period of 2 years and 8 months after the passage of the bill, to gain 2 feet in depth for an expenditure on the part of the Government of $2,000,000." "The North and South jetties placed according to the official plan can, by further extension into deep water and by the construction of auxiliary works, if needed, be made to develop all the depth of the channel which the nature of the locality will admit."

In view of these assurances, it may be well to look at the sequel as revealed by seven years of subsequent experience.

The bar has now barely 13 feet of water over its crest, which is nearly two miles further out in the Gulf than when the works were begun, and the conditions are by no means so favorable for success as they were in a natural state. The cost has been increased by requiring four additional miles of jetty work to be built at the outer end. Furthermore, the Government, having learned something from the hearings concerning the plans and methods of Capt. Eads, soon after re-convoked the Board of Engineers, who again modified the plans and estimates to include high-water stone jetties as being ultimately cheaper and more permanent than the mattress work, which was found to have subsided over sixty per cent., and which, if carried to the outer 30-foot curve, would cost $7,000,000, provided "the money be freely supplied." Their aggregate length would be nearly 54,000 feet or over *ten miles*.

The width between them was always a difficult question upon which Capt. Eads does not appear to have furnished information further than to say in reply to a member of Congress, who asked if they should be 12,000 feet apart:

"That is a matter I should take into serious study before I should determine what particular width and direction I should give but *I do not take it to be over 2,000 feet.*"

In this reply, he kept within the limits of similar maritime works to be found in the world, with one slight exception, where, by dredging, a depth of about 21 feet is maintained, yet the present official plan provides for a width of 7,000 feet or nearly *one and one-half miles*, which will prove an egregious blunder.

The impression which is being fostered that the present plans are those endorsed by Capt. Eads, and which gave promise of success is, therefore, erroneous, for although the details of the jetties are those proposed by him, their plan, form and position are far from according with its views.

A distinguished writer, in reviewing the results of this work to date, remarks that "after a series of careful experiments, a width of 7,000 feet was determined upon, and has so far proved entirely satisfactory."

As the history of these experiments shows that at no time was there more than a part of *one* jetty in existence, it is difficult to understand how a width of 7,000 feet between them could have proven satisfactory, as the result of careful experiments. No precedent for such construction is to be found in the annals of jetty building, since the world began. If Galveston be made the precedent, it will be an extravagant warning for future generations, as, instead of the deep and wide channel of a minimum navigable width of 2,000 feet at the surface and 600 feet at a depth of 30 feet, which a recent Board of Engineers have fixed upon as absolutely necessary for the contemplated port entrance, there will be merely the trenches bordering the flanks of the jetties and extending to a distance of a few hundred feet therefrom, produced by the lateral currents, and a middle ground with its prevailing depths of 12 to 13 feet between them. Jetties this far apart will be inert, and it would require two years to dredge a channel such as the above, even if the sea were to stand still and cease to supply sand to the bar for so long a period. Dredging has been tried and pronounced a failure, and no protection could be fur-

nished to a dredged channel, even if created, by jetties as now proposed, a mile and three-eighths apart, open to the storms and connected with the shore. It will be observed that the present width between jetties is a little more than half that originally considered necessary for the protection of the city.

Again, the fact that the volume of water passing out at each tide "exceeds by about 30,000 cubic feet a second the entire low-water discharge of the Mississippi River," is not reassuring as to the success of the work, since at the latter place the flow is practically constant, whereas at Galveston there must be an equal volume flowing *into* the bay. (The small land drainage being mainly consumed by evaporation does not sensibly affect the outflow.) There is, therefore, the very material difference that at Galveston the movements are virtually equal in volume, but opposite in direction, while they differ only slightly in velocity. Hence, the effects are due to the differentials between flood and ebb, while at the South Pass there is not this opposition of material forces, but a nearly constant outward discharge. If *two* jetties are built at Galveston sufficiently close to produce a scour by reaction between them, they will so reduce the influx of the tide as practically to destroy the only force available, and the problem becomes indeterminate. The only hope for Galveston is by a single jetty on the North flank of the bar, with possibly a short groin at Fort Point.

In the late animated discussion in Congress * concerning the special appropriation of $6,200,000 to complete the work under Government supervision and plans, it was stated that the people of Galveston accepted Mr. Eads' proposition to guarantee 30 feet of water across the bar for $7,500,000, and asked Congress not to give them any more money to be expended by Government engineers, in the hope that it would appropriate a lump sum, as it had done for the Mississippi, to be expended by Capt. Eads. "But Congress did not close with that proposition, and the engineers

* *Cong. Record*, March 30, 1890, p. 2874.

adopted nearly Eads' plan. They would not adopt it entirely, and whether we get deep water or not we shall at any rate have made an honest effort to do it, and if we fail, the failure will be such that its repetition will not be invited. For that reason, Mr. President, I think we ought to pass this bill."

This quotation is sufficient to show the animus of Congress in relation to these public works, of so great importance to the country. It is to remove the odium which has been placed upon this honorable body, for the failure of many of these works, on the plea of withholding appropriations. In this case at least the *onus* will be removed to the executive department having charge of the works, and it is risking nothing to say that unless the present plans are materially modified, this large sum will merely have benefited the workmen and others employed or interested in executing the project. The extension of the South jetty has intercepted the littoral drift moving southward, building a shoal in the site of the old channel and causing a rapid progression of the bar seaward, which alone has added to the cost of the work more than the price of the jetty without increasing materially the depth of the outer bar.

The last report, made by the late civil assistant engineer, states "that upon the crest of the outer bar there has been a pretty general shoaling of from one-fourth to one-half a foot. The depth in the jetty channel, however, still remains at 13¼ feet, but the bar has advanced very perceptibly gulfward.

* * * "The shoal immediately North of the jetty has continued its gulfward advances. * * * The indications are that the probable effect of the further building of the South jetty will be the obliteration of the jetty channel itself. * * *

"The gulfward advance of the bar is not an unexpected result of the extension of the jetty, and I think we may expect a continuation of this advance until the equilibrium has been restored, or the supply of sand exhausted. * * *

"The sand has accumulated on the North side of the

jetty and some has passed over the crest and settled next to the base. This is another indication of the loss of force in the littoral current at that point, and also shows from which direction the sand is moving that has caused the shoaling."

The foregoing report, stating the facts so strongly, was only produced by order of the House of Representatives, and the sequel of its publication was an order to discharge its author from further service on Government works, after about eighteen years of intelligent and faithful service at this harbor.

The problem is not, however, hopeless; the North jetty should be built at once, but of such form and in such position as to control the prevailing natural elements which maintain the bar.

It is said that time and money will remove mountains, and while the aphorism is true of mountains, *in situ*, it cannot be applied to those in motion where the incessant forces of nature are continually constructing a path whereby they may traverse a breach in the coast. The remedy is not to be found in twin jetties, for such physical conditions as exist at Galveston.

Thus, after more than twenty years of experiments, frequent changes of commanding officers, several modifications of plans, the expenditure of $2,273,111.66 to June 30, 1890, and more than a quadrupled estimate of cost to complete, it may be said that the injuries caused by the works are greater than the benefits, and that the difficulties in the way of securing a deep water channel over the outer bar have been greatly increased rather than diminished, while it is proposed to apply $6,200,000 to a continuation of these experiments on a plan which must prove fatal. Such is believed to be a frank, though greatly abridged, statement of the problem of securing deep water at Galveston, as it exists to-day.

NEW CONSTRUCTIONS OF THE FORCE OF INERTIA OF CONNECTING RODS AND COUPLERS, AND CONSTRUCTIONS OF THE PRESSURES ON THEIR PINS.

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[*Concluded from p. 206.*]

G.

We will now take up the general case of the quadric chain *ABCD*, *Fig. 8*. Here both cranks (or levers) *AB*, *DC*, have variable angular velocities. Let *AO* equal the acceleration of pin *A* in direction and intensity. It is required to find the acceleration of pin *D* for the given position and motion of the mechanism. For this case we will at once give both construction and proof.*

The velocity $\overline{Av} = \overline{AH}$ is found like the velocity $\overline{CV}$ in *Fig. 3*. On $\overline{AB}$ as a diameter we describe a circle. This cuts at *e* the perpendicular *Of* dropped on crank *AB* from the end *O* of the given acceleration *AO*. Then will $Ae = AH = Av$ be the velocity of pin *A*, because *Af* is the normal component of the acceleration, and the equation,

$$\overline{Af} = \frac{v^2}{R} = \frac{\overline{Ae}^2}{\overline{AB}}$$

is satisfied by the construction just given. (When point *f* falls beyond *B* outside of *AB*, then this construction fails

* *Fig. 8* is, in the main, like *Fig. 784, Plate LII*, in Dr. Burmester's *Lehrbuch der Kinematik*. The construction and proof given above differ somewhat from that given there, but both are suggested by Dr. Burmester's treatment of the subject. The reader will find many interesting special cases in this masterly, German, work, and a wealth of constructions. But the method of finding the acceleration of the slide given in *Figs. 1, 2* and *3* of the present article was not used there. A series of lines, in principle like the broken lines *WVB'TSw* of *Fig. 3*, and *DHJTSd* of *Fig. 8* was used there instead. This series is much simpler than any former method, but fails at the dead centre and gives poor *T* intersections when crank is near that centre.

and we must lay off $Af' = Af$ beyond A, and on Bf' as a diameter construct a circle; this will cut the tangent to the path of A at v and Av will be the desired velocity, for this construction also satisfies the equation.)

To find the velocity of point D, we draw HI parallel to AD, and because P is the instantaneous centre of link AD and

$$\frac{AH}{DI} = \frac{AP}{DP} = \frac{\text{velocity of pin } A}{\text{velocity of pin } D}$$

the lengths DI will measure the velocity of D. As each of

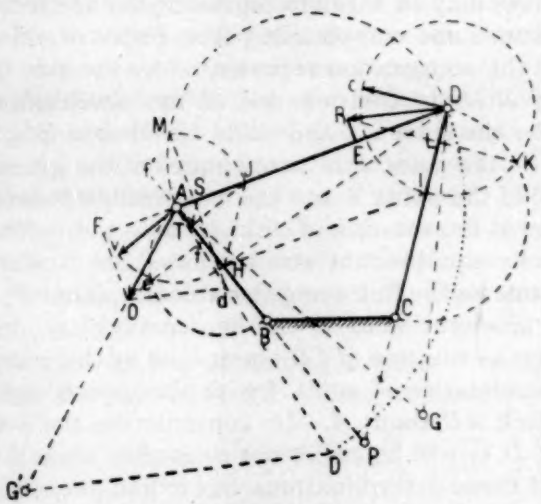

FIG. 8.

the instantaneous radii PA, PD makes the same angle (90°) with the direction of the velocity of the point to which it is drawn, they may be taken as the representatives of the velocities of the points A and D. Since velocities may be combined and resolved like forces, we may regard APD as a triangle of velocities, and either of its sides as the resultant of the other two. Thus PD may be regarded as the resultant of PA and AD. That is, the velocity of point D may be considered as the resultant of the velocity of point A about P and of the velocity of point D about A as a centre (the

angular velocity about A being then equal to that of A around P).*

Since AH represents the velocity of A and $HJ = DI$, the velocity of pin D, the sides of the triangle AHJ can replace the sides of APD as the representatives of the velocities. The length AJ thus represents on our scale the aforesaid velocity of point D of the rod when turning about its point A.

Let G represent the centre of acceleration of the link or coupler AD. The acceleration of A is proportional to the radius AG and that of D to the radius DG, and as these accelerations make constant angles with the radii, the radii themselves may be taken to represent the accelerations. As accelerations are compounded like forces or velocities, we see that the acceleration represented by the side GD of the triangle AGD is compounded of the accelerations represented by the sides AG and AD. In other words, the acceleration of the point D is compounded of the given acceleration AO of the point A and the acceleration possessed by D by virtue of the rotation of link AD about A (with an angular velocity equal to that which it has about G, which in turn is the same as the link's angular velocity about P).

The acceleration of D can be conceived as determined either by the rotation of CD about C, or by the compounding of the acceleration of point A with that caused by the rotation of link AD about A. In constructing the acceleration of point D, it will be easier not to confine oneself wholly to either of these determinations, but to find one component by each of these methods. For example: In the case before us, we find the radial component DL along DC and also the resultant radial component DE $(= FA + AS)$ along DA and erect the perpendiculars LR and ER at their extremities. As each of these perpendiculars necessarily passes through the end R of the total acceleration of D, their intersection will determine R and consequently the desired value DR.

* This can also be regarded as an example of the resolution of a rotation about an axis P into an equivalent rotation about another parallel axis A *plus* a translation that is itself equal to the absolute, circular motion of axis A.

The radial component of the total acceleration of D due to the rotation of CD about C is

$$DL = \frac{\overline{DI^2}}{\overline{CD}}$$

which can now be computed, or it can be constructed by drawing two circles, one from the centre D with radius DI, the other on CD as a diameter, and joining their intersections by a straight line, which will cut CD in the required point L.

To find that component of the total acceleration of D which lies along link DA we must remember that the motion of D is the resultant of the motion of A and the rotation of link DA about A with an angular motion equal to that with which DA rotates about instantaneous centre P. The component of the total acceleration of A which lies along DA is evidently AF and that which is due to the rotation of DA about A is AS and is found in the same manner as DL. We found above the velocity of D about A to be AJ. To get AS we draw two circles, one on AD as a diameter and another with AJ as a radius, and join their intersections; the connecting line will cut AD in the desired point S. This satisfies the condition $\overline{AS} = \overline{AJ^2} \div AD$.

There is another way of finding S when P is accessible. It is to draw BS parallel to PJ. Then we have

$$\overline{AS} : \overline{AJ} = \overline{AH} : \overline{AP} \quad \text{or} \quad \overline{AS} = \overline{AJ}\,\frac{\overline{AH}}{\overline{AP}} = \overline{AJ}\,\frac{\overline{AJ}}{\overline{AD}} = \frac{\overline{AJ^2}}{\overline{AD}}$$

There is still another way of finding S, one that is generally available, and that is to join D and H and draw through J a parallel JT to DH, and from T a parallel TS to CD. To prove this we have

$$\overline{AJ} \times \overline{AT} = \overline{AS} \times \overline{AH} \quad \text{also} \quad \overline{AJ} \times \overline{AH} = \overline{AT} \times \overline{AD}$$

multiplying these equations and cancelling we get

$$\overline{AS} = \overline{AJ^2} \div \overline{AD}$$

We now have the component acceleration FS ($= FA + AS$) which acts along the link AD. Make $DE = FS$ and at

E erect a perpendicular *ER* to *AD*. This will be one locus of the end *R* of the desired acceleration of *D*. The perpendicular *LR* to *CD* has already been stated to be a locus and the intersection of the two loci will then completely determine *DR* as the acceleration of the point *D*.

If the direction of *DR* had been known, the problem would have been greatly simplified, for it would only have been necessary to draw through *O* a line *Od* parallel to the given direction *DR* and the intercept *Od* between *O* and the chord *MSd* would have given at once the intensity of the acceleration of *D*. This construction is directly applicable to the slider crank, greatly simplifying that particular case.

The construction given above for the quadric chain is applicable to a still more general case, namely, to a case in which the points *A* and *D* of link *AD* do not move in arcs of circles, but in any curves φ and λ whose centres of curvature are known. These centres may be put in place of the fixed centres *B* and *C* used above and then *BA* and *CD* will act as cranks of a quadric chain for two successive instants and the whole construction will be like that already given.

H.

We will now apply this general construction of the acceleration of the second pin *D*, when that of the first pin *A* is given, to the special cases represented by *Figs.* 7, 3 and 1.

In the quadric chain there is a particular case that often arises in practice, that in which the crank *AB* rotates with uniform velocity. As the crank radius itself can then represent completely both velocity and acceleration of pin *A*, the general construction is simplified, for it is not necessary to find point *H* (*Fig.* 8). But this can be reduced to the still more simple method of *Fig.* 7. As this is at the same time a symmetrical construction, it can be easily remembered. The proof is as follows:

The triangle *BAJ* is equal to the triangle *DHI*, and their homologous lines are equal and parallel. Then $VU = ED = FS$. By the general method the two normals *LR* and *UR* determine at once the acceleration *DR* of the pin *D* when the acceleration of crank-pin *A* is represented in direc

tion and intensity by crank radius AB. Now we have the proportion $DI : JN = QI : QN = QC : QB = CD : BJ$, hence

$$\overline{IN} = \frac{\overline{BJ} \times \overline{DI}}{\overline{CD}} = \frac{\overline{DI}^2}{\overline{CD}} = \overline{DL}.$$

This makes $BN = IL$, and since by construction $\overline{IU} = \overline{SJ} = \overline{BO}$, we have the quadrilateral $BNrO$ equal to the quad-

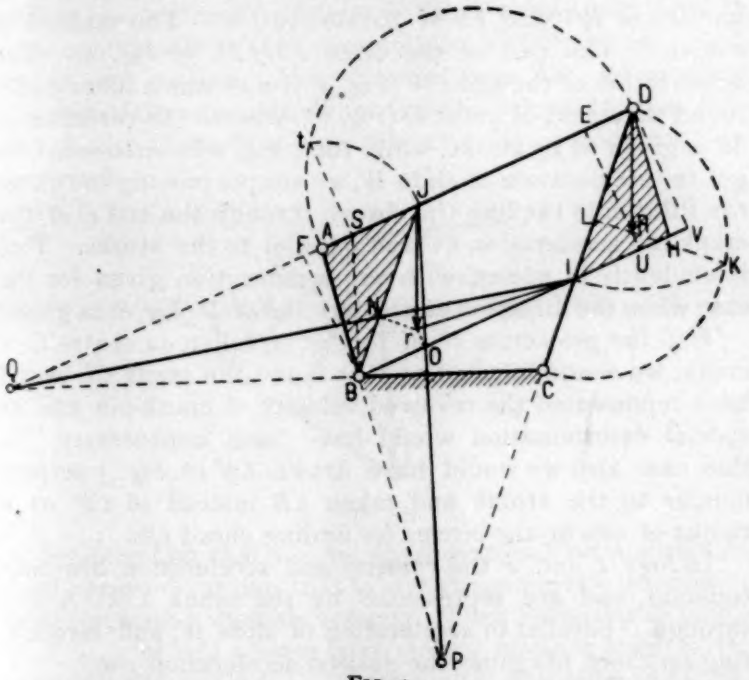

FIG. 9.

rilateral $ILRU$. As N and O are easily found, we can readily determine the length Jr, which is equal and parallel to DR, the required acceleration of the point D. This simplified and symmetrical construction has already been given with *Fig. 7*.

When the quadric chain $ABCD$ *Fig. 8* reduces to the slider-crank $LCWM$ of *Fig. 3*, the sides BC and CD of *Fig. 8* become parallel to each other, infinitely long and perpendicular to the stroke WN of *Fig. 3*. We construct the

revolved crank-pin velocity *CV* (*Fig. 3*) exactly in the same manner as *AH* of *Fig. 8*. Now through *V* draw *VB'* (*Fig. 3*) perpendicular to the stroke, for this is equivalent to drawing *HJ* parallel to *CD* in *Fig. 8*. The point *S* (*Fig. 3*) is found exactly as in *Fig. 8* (*i. e.*, a circle is described from *C* with *CB'* as a radius, and a second circle on the rod as a diameter) and chord *tSv* is then drawn. A second method of finding *S* is given by the series of lines *WVB'TS* where *B'T* is parallel to *WV* and *TS* is parallel to *VB'*. The method is evidently like that of the lines *DHJTS* of *Fig. 8*. The acceleration of the slide *W* (*Fig. 3*) is now much more easily found than that of point *D* (*Fig. 8*), because the direction of *W* is given in its stroke, while that of *D* was unknown. To get the acceleration of slide *W*, we simply prolong the chord *tSv* till it cuts the line *Ow*, drawn through the end *O* of the crank-pin acceleration *CO* and parallel to the stroke. This is evidently in accord with the construction given for the case when the direction of acceleration of *D* (*Fig. 8*) is given.

Had the projection *O'* of *O* (*Fig. 3*) fallen on centre *L* of crank, we would have point *V* at *L* and the crank *CL* would have represented the revolved velocity of crank-pin and its special determination would have been unnecessary. In this case also we would have drawn *LB* in *Fig. 3* perpendicular to the stroke and taken *CB* instead of *CB'* as a radius of one of the circles for finding chord *tSv*.

In *Figs. 1* and *2* the velocity and acceleration are both constant, and are represented by the crank *CO*. A line through *O* parallel to acceleration of slide *W*, and terminating on chord *tSv* gives the desired acceleration *Ow*.

We have thus far supposed the acceleration of one pin of the rod or coupler completely known, and constructed the acceleration of the other pin. This enables us to find the acceleration of every point of a moving system like the coupler of the quadric chain, or the connecting rod of the slider-crank mechanism. For if the acceleration of two points of a plane system moving in its own plane are known, we can find the centre of acceleration of the moving system, and then the acceleration of any point of the system will be directly proportional to its instantaneous radius

from that centre, and the angle which its direction makes with this radius will be the same as that made by the other points with their respective radii. When the centre of acceleration of a system is known, and the direction and intensity of the acceleration of one point of its system is also given, then the accelerations of every other point can be easily and completely determined.

Our next step will be to establish simple methods of finding the centre of acceleration* and then apply these or their equivalents to the constructions already given. *Figs. 10, 11* and *12* are taken directly from Dr. Burmester's *Kinematik*, already referred to, and the demonstrations connected with these figures are likewise taken from this German work.

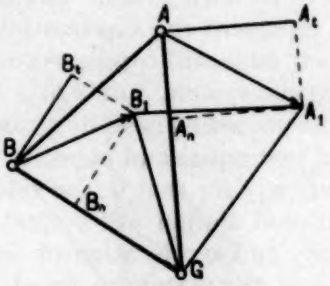

FIG. 10.

Suppose first that a plane, rigid, system S rotates about a fixed centre G (or one that is stationary for two successive instants), with a variable velocity. Then because the points $A, B, \ldots$ of this system have similar motions about G, the triangles $GAA_1, GBB_1, \ldots$ are similar, the sides $AA_1, BB_1, \ldots$ representing the acceleration of the points $A, B, \ldots$ consequently the triangle GA_1B_1 is similar to GAB. If we join the ends $A_1, B_1, \ldots$ of the accelerations by straight lines, and call the system which these form S_1, we can also say that *systems* S_1 and S *are similar, and the centre* G *is a point common*

* *Weisbach-Herrmann's Machinery of Transmission*, Introduction §§ 15–21, inclusive, give methods of finding this centre, of which that given in § 21 is the most convenient. But still simpler methods are available as the determinations in *Figs. 1, 2* and *3* have shown.

to both. Another way of putting it would be to say, that the *ends* of the accelerations together form an *image* that is similar to the original system, and that the centre G contains a pair of corresponding points.

It is evident that in this case the acceleration of one point being known we can easily find that of every other point.

If we suppose each of the accelerations AA_1, BB_1, . . . to be resolved into its normal and tangential components, AA_n, AA_t, . . . ; BB_n, BB_t . . . ; then will the system GA_nB_n, GA_tB_t be similar to the rotating system GAB, and the centre G will contain a pair of their corresponding points.

A more general case is where a plane system S has any arbitrary motion in its own plane. Let the distances AA_1, BB_1, CC_1, *Fig. 11*, represent the accelerations of the points of this system. Their ends will form a system $A_1B_1C_1$; that is, similar to the original system S or ABC. To prove this, we will reduce it to the preceding case, by giving to the system as a whole a motion just equal and opposite to that possessed by one of its points, say A; that is, the velocity and acceleration of this additional motion will be just equal and opposite to the velocity and acceleration of point A. This will cause the point A of the system to be at rest for two successive instants, and system S will turn about this fixed point A during these two intervals. Combine the additional and equal accelerations AA_1', BB_1', CC_1' with AA_1, BB_1, CC_1, respectively, by means of the parallelogram of accelerations. For point A the resultant will be zero, for point B it will be BB_1'' and for point C it will be CC_1''. Now we know from the preceding case because of this resultant rotation about A, that triangle $AB_1''C_1''$ will be similar to ABC. But because B_1B_1'' is equal and parallel to BB_1', and this in turn to AA_1, we have $AA_1B_1B_1''$ a parallelogram, and, therefore, A_1B_1 equal and parallel to AB_1''. In the same way we can show that $AA_1C_1C_1''$ is also a parallelogram, and, therefore, A_1C_1 equal and parallel to AC_1''. This will make triangle $A_1B_1C_1$ equal to triangle $AB_1''C_1''$, and as the latter is similar to ABC we also have $A_1B_1C_1$ similar to ABC, which was to

be proved. We can now enunciate the following proposition:

The ends of the accelerations of the points of a plane system having con-plane motion constitute a plane system (or image) that is similar to the first or moving system.

If we know the accelerations AA_1, BB_1 (*Fig. 11*), of two points A, B, of the moving system, the condition of the whole system with respect to acceleration is known. For we can get the acceleration belonging to any point C whatever, by constructing on A_1B_1 as a base a triangle $A_1B_1C_1$ that is similar to ABC. Moreover, two similar systems have only one common point that contains a pair of corresponding points. This common point is G, and is called the centre of acceleration. We will now proceed to prove the proposition italicized below.

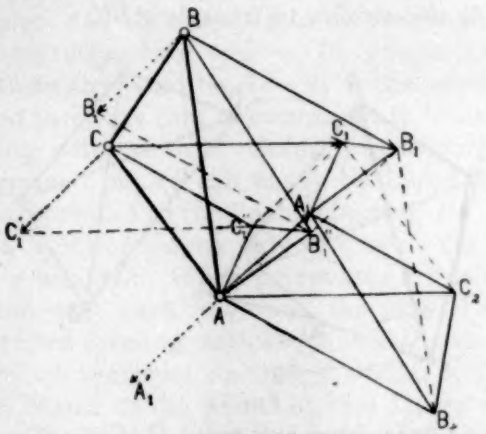

FIG. 11.

If, in *Fig. 11*, we draw AB_2 equal and parallel to BB_1, and AC_2 equal and parallel to CC_1, we shall have triangle $A_1B_2C_2$ similar to triangle ABC. To establish this we start with what was proved above, namely, that triangles $AB_1''C_1''$ and $A_1B_1C_1$ are equal to each other, have their sides parallel, and are similar to ABC. Then we must show that triangles ACC_1'' and ABB_1'' are similar. This is so because they have an angle CAC_1'' of the one equal to the angle BAB_1'' of the other, and $AC : AC_1'' = AB : AB_1''$. Tne next step is

to show that triangle $A_1C_1C_2 = ACC_1''$, and triangle $A_1B_1B_2 = ABB_1''$, and hence because the triangles in the last members of the equations are similar, those in the first members $A_1C_1C_2$ and $A_1B_1B_2$ are also similar. That $A_1C_1C_2 = ACC_1''$ is true, because the two sides, A_1C_1 and C_1C_2 of the one are respectively equal and parallel to the two AC_1'' and AC of the other, and the angles included by these sides are equal. In like manner it may be shown that $A_1B_1B_2 = ABB_1''$. We may, therefore, conclude that $A_1B_1B_2$ and $A_1C_1C_2$ are similar triangles. The next step is to prove triangle $A_1B_2C_2$ similar to $A_1B_1C_1$. Because the triangles $A_1B_1B_2$ and $A_1C_1C_2$ are similar, we have $A_1B_1 : A_1C_1 = A_1B_2 : A_1C_2$, and angle $B_1A_1B_2 =$ angle $C_1A_1C_2$. Taking from these equal angles the common angle $B_1A_1C_2$, we have angle $B_1A_1C_1 =$ angle $B_2A_1C_2$, and hence triangles $A_1B_2C_2$ and $A_1B_1C_1$ are similar. This makes triangle $A_1B_2C_2$ also similar to triangle ABC.

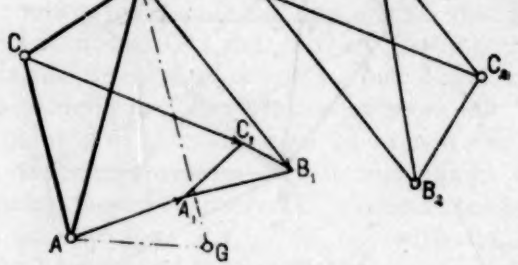

FIG. 12.

Now let us draw from any point O (*Fig. 12*) as a pole, the vectors OA_2, OB_2, OC_2, respectively equal to, similarly directed and parallel to, the accelerations AA_1, BB_1, CC_1 of the points A, B, C, of a plane system S, then will the points $A_2B_2C_2$ constitute a plane system S_2, which is similar to the original one S. Hence the proposition:

If from any assumed point as a pole we draw vectors parallel to, similarly directed and equal to, the accelerations of the points of any plane system having con-plane motion, the ends of these vectors will form a plane system (or image) which is similar to the moving plane system.

Finally, by constructing in *Fig. 12* the triangle ABG similar to the triangle A_2B_2O, we get in G, the desired centre of acceleration. It will not always be necessary to find this centre G in order to find the accelerations of all the points of the system, for it may often happen that the pole O may be assumed as to give the desired result more quickly and conveniently. *Figs. 1*, *2* and *3* are good illustrations of this.

To apply this to our series of figures we will begin with the quadric chain, *Fig. 8*, and seek the centre of acceleration, G, of the moving system or link, AD. Here we know two accelerations, AO of pin A and DR of pin D. Take the end of the acceleration of A for the assumed pole, and draw from it Od, parallel, similarly directed and equal to DR. Then Ad may be regarded as a similar and reduced image of the coupler AD. We may take any row of points on AD, and a corresponding row on its image Ad, and the vector drawn to any point on Ad will be the acceleration in direction and intensity (but of course not in location) of the corresponding point on AD. Points on Ad corresponding to those assumed on AD can easily be found by drawing lines from AD parallel to the line connecting Dd. This last construction holds of course only so long as the points are on the centre line AD. When points are off this centre line we join them with each other and the pins A and D, the lines thus drawn forming with the link AD a closed figure. Then on Ad we construct an image similar to this figure. The vectors drawn to the points of this image will be the accelerations in direction and intensity of the corresponding points of the figure on AD.

The centre of accelerations G can be easily found from the directions given above. We have only to construct a triangle $G'AD'$ similar to υAd, but with side AD' equal to the link AD. For this purpose prolong Ad till $AD' = AD$, then draw through D' a line $D'G'$ parallel to dO till it cuts AO prolonged in G'. Now about A as a centre, revolve the triangle $AG'D'$ till AD' coincides with link AD the vertex G' will then move to G, and this latter point will be the desired centre of acceleration. It is evident that the ray

makes GA the angle GAO with the acceleration AO of A, and as this is the same at every point, we see that in this case the image deviates in position from its original an amount equal to the angle between the acceleration of any point and its instantaneous radius of acceleration.

In *Figs. 3, 2* and *1* there is no need of finding the centre of acceleration. In each of these O is the pole and Cw the image of the centre line CW of the rod. In *Fig. 3* the triangle $Cwgh$ is the image that is similar to the triangular

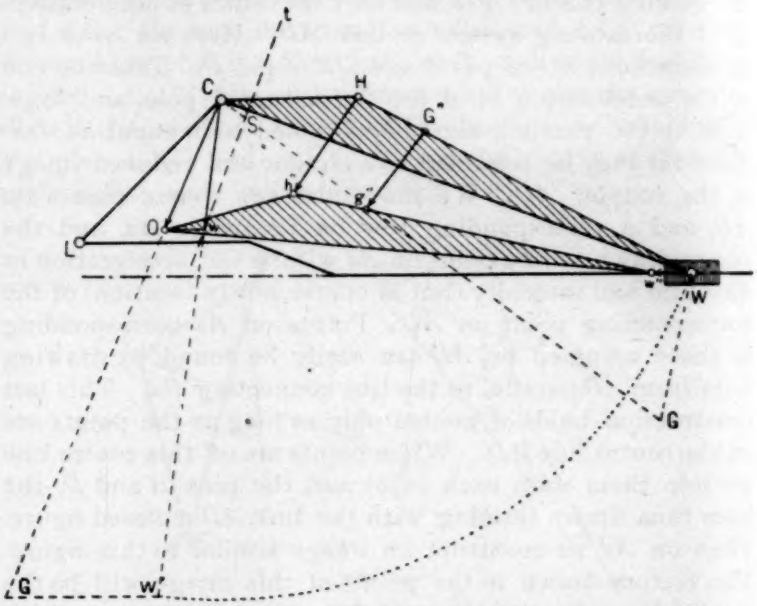

FIG. 13.

figure $CWGH$ on the rod. The vectors Og, Oh to the points g, h of the image give exactly the accelerations of the corresponding points G and H of the rod. The vectors give exactly the direction of the acceleration, because in each case the image makes with the figure, exactly the angle of acceleration. If for any reason the centre of acceleration of the rod is desired in any of these figures (say *Fig. 3*), it can be very easily found by prolonging Cw (*Fig. 13*) till it is equal to the rod $CW' = CW$, then through this point W'

draw a horizontal till it intersects (say at G') the crank prolonged in *Figs. 1* and *2*, or till it intersects the line CO in *Fig. 3* or *Fig. 13*. Then revolve this triangle $CW'G'$ about C till CW' coincides with its equal, the rod CW. The position G which this vertex G' then occupies will be the centre of acceleration desired.

That the ends of the accelerations form an image that is similar to the figure $CHG''W$ is also illustrated by the shaded area $Oh''g''w''$ shown in *Fig. 13*.

PROCEEDINGS
OF THE
CHEMICAL SECTION
OF THE
FRANKLIN INSTITUTE.

[*Proceedings of the stated meeting, held Tuesday, September 15, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, September 15, 1891.

Dr. W. H. WAHL, President, in the chair.

Thirteen members and two visitors were present.

Mr. Pemberton nominated Mr. W. N. Allen, No. 2 Chestnut Street, Philadelphia, for membership in the Section. Owing to the absence of a majority of the committee on admissions, action upon the nomination was deferred.

Mr. Pemberton reported that Mr. Henry Bower and himself were present at the meeting of the Committee of the American Chemical Society, held during the past summer, in Washington, for the purpose of considering the establishment of a national chemical organization. He reported progress, and the committee was continued.

A number of bills, presented by the Actuary of the Institute, were considered by the Section, and the President was instructed to approve them for payment.

A paper by Mr. Cabell Whitehead, of the Mint Bureau in Washington, on "The Use of Cadmium in Assaying Gold Bullion," was read by the Secretary in the absence of the author. Dr. Tuttle commented upon the paper and discussed the most important advantages of the proposed method.

Mr. Waldron Shapleigh, of the Welsbach Incandescent Gas Light Company, of Gloucester, N. J., exhibited a series of specimens of salts of the rare earths. The collection was unique and of great interest and value. No such complete collection of these rare bodies was ever exhibited before a scientific body in this country. Mr. Shapleigh discussed the sources of the

minerals from which the salts were obtained, and also the methods of purification used.

The exhibition was highly appreciated by the members present.

The President of the Section tendered thanks to the exhibitor for the rare opportunity he had afforded the members of the Section. (Appended is a list of the preparations exhibited.)

Adjourned. WM. C. DAY, *Secretary.*

Mr. Waldron Shapleigh exhibited the following specimens of salts of the rare earths:

Praseodymium, neodymium and lanthanum oxides, sulphates, nitrates, chlorides, carbonates, oxalates, acetates and double salts with the alkaline metals.

Cerium oxide, oxalate, chloride, nitrate, and the double nitrate of the cerous and ceric oxides with ammonium.

Yttrium and erbium nitrates, oxides and oxalates.

Zirconium oxide, nitrate, sulphate and some double salts.

Yttrium and erbium (not separated) oxides and nitrates obtained from gadolinite, cerite, monazite, fergusonite and samarskite. Thorium and vanadium salts.

Also large specimens of the following minerals from which these salts were obtained: samarskite, zircon crystals and monazite sand from North Carolina, monazite sand from Brazil, gadolinite from Texas and allanite from Virginia.

Mr. Shapleigh said the collection was of interest, as it is the first time the salts of praseodymium and neodymium have been shown, and probably separated in this country. Some of the salts have not been heretofore prepared.

The separation of these elements is long and tedious; the specimens shown having undergone nearly 400 fractional crystallizations, and having been in state of constant preparation since early in 1888. Tons of cerite and monazite sand have been used, and tons of the salts of cerium and lanthanum obtained, but the yield of praseodymium oxide has been only a few kilos. The percentage of neodymium is much higher.

Dr. Carl Auer von Welsbach, in 1885, was the first to separate didymium into these elements, and together with Prof. Bunsen, to determine their atomic weights; that of Pr 143·6 and of Nd 140·8. The oxides are M_2O_3 and probably M_4O_7.

With one exception, the salts of praseodymium exhibited were of a pale green, and of neodymium pink or amethystine color.

Zirconium, lanthanum and cerium should no longer be classed among rare earths, as hundreds of tons of ores from which they are obtained have been located in North Carolina, and there seems no end to the deposits of monazite sand, one of the richest ores, and containing most of the rare earths. In Brazil it does not have to be mined, as it is in the form of river sand. In North Carolina it is found in washing for gold.

Should the arts, trades or manufactures create a demand for these so-called rare earths, nature could readily supply it from these two localities.

Thorium and yttrium minerals are not so easy to obtain; they have, however, recently been found in quantity in North Carolina and Texas.

Working on a commercial scale, he finds the yield of lanthanum from cerite nearly one per cent. higher than stated in the analyses published.

CHESTNUT-WOOD TANNIN.

By Prof. Henry Trimble.

[*Read at the stated meeting of the Chemical Section, held June 16, 1891.*]

The *Castanea Vesca* of Linnaeus or *C. Sativa* of Miller, variety *Americana*, is a large tree of rapid growth, found in many parts of the United States, especially in the eastern section from Maine to Delaware and on mountains as far south as northern Alabama.

An extract of the wood and bark has been prepared in this country and France for a number of years. This extract is especially useful in tanning where it corrects the reddish color of hemlock, and in dyeing where it gives a dead black with salts of iron. It must not be confounded with the extract of chestnut oak, *Quercus Prinus*, Linn., which is also largely manufactured in this country, and, no doubt, the manufacturers are not very particular to keep the two separate.

Most of the literature on the subject of chestnut tannin refers to that from the horse chestnut, *Æsculus Hippocastanum*.

In order to leave no room for doubt about the origin of the tannin described in this paper, the chips, free from bark, were collected from a large tree about forty years old, cut in the month of August.

A summary of the constituents of the wood, as found by a proximate analysis, may be of interest before commencing a description of the tannin:

	Per Cent.
Crystalline wax, melting at 50° C., soluble in hot 95 per cent. and in absolute acohol,	1·03
Gallic acid,	·05
Resin,	·28

	Per Cent.
Tannin, extracted by absolute alcohol,	3·42
Mucilage,	1·15
Dextrin,	1·89
Sugar,	·96
Tannin, extracted by water,	1·92
Pectin and albuminoids,	1·46
Extractive, dissolved by dilute acid,	2·95
Ash,	7·08
Moisture,	7·05
Cellulose and lignin,	70·76
	100·00

It is not certain that the gallic acid pre-existed in the wood, because it may have been formed during the drying of the chips, since the analysis was not made for some time after they were cut and ground.

A special determination of tannin on a separate portion of the wood by gelatin and alum and by permanganate and hide powder gave, respectively, 7·86 and 7·85 per cent.

The tannin used in the following experiments was prepared by percolating two and one-half kilos of the finely powdered wood with a commercial ether, which consisted of about seventy-four parts ether, twenty-six parts alcohol and a small quantity of water. The ether was recovered from the percolate by distillation and the residue dissolved in cold water. The filtered aqueous solution was precipitated in three portions by lead oxy-acetate, the precipitates decomposed by hydrogen sulphide, the latter removed by distillation under reduced pressure, and the solution after cooling agitated with ether, which extracted gallic acid from each of the three fractions. The tannin solution from the middle fraction was warmed to remove ether and then saturated with common salt. An abundant separation of tannin took place, which was collected, washed with a saturated solution of salt, and dried over sulphuric acid in a vacuum.

The dry residue was dissolved in ether-alcohol, filtered and rapidly evaporated to dryness under diminished pressure, which left the tannin in a porous, light-red mass. This was used for some of the qualitative tests, but for many of the reactions the tannin was further purified by

again precipitating in three fractions with lead oxy-acetate, the lead removed by hydrogen sulphide, the latter removed by distillation under reduced pressure, until the liquid had reached a small bulk, when it was agitated with ether, which removed gallic acid from each of the three fractions. The aqueous solution was then distilled to dryness under reduced pressure, the residue in each case dissolved in ether-alcohol, filtered and rapidly distilled under the same circumstances to dryness, which left the tannin in a nearly white porous mass completely soluble in cold water.

In physical and chemical properties this chestnut-wood tannin so closely resembled gallotannic acid that the following comparative statement is given, to show both the character and resemblance at once:

Reagent.	*Chestnut-wood Tannin.*	*Gallotannic Acid.*
Ferrous salts,	No change,	No change.
Ferric chloride and Ammonium hydrate,	Blue-black ppt., Purple ppt.,	Blue-black ppt. Purple ppt.
Tartar emetic and Ammonium chloride,	Slight clouding, Pale ppt.,	Slight clouding. Pale ppt.
Copper sulphate and Ammonium hydrate,	No ppt., Light-brown ppt.,	No ppt. Light-brown ppt.
Bromine water,	No ppt.,	No ppt.
Calcium hydrate,	White ppt. turning light blue,	White ppt., turning light blue.
Ammonium molybdate,	Yellow color,	Yellow color.
Sodium sulphide,	No change,	No change.
Conc. sulphuric acid,	Light yellow,	Light yellow.
Sulphuric acid (1 to 9),	No deposit,	No deposit.
Lead nitrate,	White ppt.,	White ppt.
Cobalt acetate,	Flesh-colored ppt.,	Flesh-colored ppt.
Manganese acetate,	White ppt.,	White ppt.
Uranium acetate,	Crimson color, turning dark red,	Crimson color, turning dark red.
Ammonium picrate,	No ppt.,	No ppt.
Potassium bichromate,	Brown ppt.,	Brown ppt.
Ferric acetate,	Blue-black color and ppt.,	Blue-black color and ppt.
Alkaloids,	White ppt.,	White ppt.
Gelatin,	White ppt.,	White ppt.
Lead acetate,	Light ppt.,	Light ppt.

One per cent. solutions of the tannins were used as recommended by Procter,* and all the reagents were applied in solution except sodium sulphide. These reactions do not all agree with those given by Procter, but he used a solution of commercial extract of chestnut which may have consisted in part or entirely of chestnut oak, or the difference may be due to the non-tannin constituents of the extract.

The chestnut-wood tannin was found to decompose, when heated to 200° C., into pyrogallic and metagallic acids, an abundant crop of crystals being obtained of the former. Each of the three fractions was estimated for sugar and the first found to contain 10·48, the second 7·98 and the third 6·18 per cent. of glucose, which, no doubt, had existed as a glucoside.

The second fraction was dried at 120° C. and submitted to elementary analysis as follows:

(1) ·1179 grams of substance gave ·2211 CO_2 and ·0486 H_2O.
(2) ·0843 grams of substance gave ·1575 CO_2 and ·0363 H_2O.

	(1)	(2)
C,	51·15	50·95
H,	4·58	4·79
O,	44·27	44·26
	100·00	100·00

On calculating for the presence of 7·98 per cent. glucose and deducting the difference from (1), we get:

	Found.	*Calculated for* $C_{14}H_{10}O_6$
C,	52·11	52·17
H,	4·40	3·10
O,	43·49	44·73
	100·00	100·00

It will be noticed that the percentage of hydrogen is greater than that in digallic acid, but in view of the fact that it was dried at 120° C. instead of 140° C., as recommended by Löwe,† and that it agrees so closely with gallo-

* *Text-Book of Tanning*, p. 112.

† *Zeitschrift für Anal. Chemie*, **11**, 378.

tannic acid in all its reactions, we cannot but conclude that it is gallotannic acid.

An acetyl derivative was prepared which in many respects resembled pentacetyl tannin, but the figures proving it could not be obtained in time for this paper.

PROCEEDINGS

OF THE

ELECTRICAL SECTION

OF THE

FRANKLIN INSTITUTE.

[*Proceedings of the stated meeting, held Tuesday, September 8, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, Sept. 8, 1891.

Prof. EDWIN J. HOUSTON, President, in the chair.

Present, twenty members and visitors.

The minutes of the meeting of June 2d were read and approved.

The treasurer presented bills for printing and clerical work, which were ordered paid when approved.

Six nominations to membership were referred to the committee on admissions.

Prof. Edwin J. Houston read a paper on "The Artificial Production of Rain." There was much discussion on this subject, and also on the queries from the Question-box regarding the comparative intensity of the magnetic north and south poles of the earth, and the results obtainable in using Edelmann's electrometer as described in Ayrton's *Practical Electricity*.

The meeting then adjourned.

L. F. RONDINELLA, *Secretary.*

ARTIFICIAL RAIN-MAKING.

By Prof. Edwin J. Houston.

[*Read before the Electrical Section of the Franklin Institute, Sept. 8, 1891.*]

Whenever a large mass of air is cooled below the temperature of its dew point, the moisture it can no longer hold as invisible vapor, becomes visible. If the reduction of temperature be but slight, the vapor appears as fog, mist or cloud; if the reduction be considerable, as rain or snow.

There has been no little attention given lately to the question as to whether or not rain can be caused to fall at pleasure on any given section of the earth—rain machines, or artificial rain producers, consisting essentially of devices whereby explosions of nitro-glycerine, or other similar substances, are obtained at fairly considerably elevations in mid-air, have been tried in different forms. As to the success of these attempts at the artificial production of rain, the testimony appears to be uncertain or contradictory.

The idea of rain-making by mid-air explosions, is probably based on the rains that are generally believed to attend or follow great battles, Fourth of July celebrations of the Chinese character, and volcanic eruptions. Passing by the evidences produced by either the warlike or the peaceful burning of gunpowder, which at best are but vague, it may be remarked that volcanic eruptions may produce very heavy rainfalls, not only because the force of the eruption and the intense heat cause upward currents in the air, but also because of the vast quantities of vapor of water that escape from most volcanoes during their eruptions.

There is a fascination in witnessing man's struggle with the forces of nature; a struggle, be it understood, not made to oppose such forces, but rather to direct them. The former effort would be foolish, the latter must meet with success, if properly directed.

Do the scientific facts, as far as known to meteorology,

give any encouragement for the continuance of the efforts of the would-be rain-makers? Let us inquire:

It is now generally agreed that the lowering of temperature necessary for the production of rain may be obtained in the following ways:

(1) By the intermingling of masses of warm and cold air.

(2) By the carrying of warm, moist air into a cold place.

In any case the cause of the rain is, briefly, the cooling of the air until it is unable to retain all the moisture it formerly held as invisible vapor, and deposits the excess in a visible form as rain.

The quantity of the rainfall will, therefore, depend both on the amount of moisture present in the air, and on the extent of reduction of temperature produced.

The first method, viz: The lowering of temperature by the intermingling of masses of warm and cold air, can never produce any very considerable rainfall, since, though the warm air is cooled by its mixture with cold air, and the tendency is, therefore, to cause the mixed air to become relatively moister, yet, at the same time the cold air is made warmer, and therefore, relatively drier. Drizzling rains might be produced in this manner, but scarcely ever heavy rainfalls, unless both the cold and the warm air contain large quantities of moisture.

There remains, therefore, but the second way of lowering the temperature of the air, viz: by the carrying of the warm air into a colder place. This can be accomplished in three different ways:

(1) By a change of latitude, or by a warm, moist air blowing into a colder latitude. In general, the equatorial currents blowing toward the poles are the chief rain producers.

(2) By a change in altitude, effected by an ascending current, due to a heated area. Here the lowering of the temperature is due not only to the cold of elevation, but also to that produced by the expansion of the air under lower pressure.

(3) By a change in altitude, due to a mountain range

opposing the progress of a wind, and thereby necessitating its gradually creeping up the sides of the mountain.

In any of these ways heavy rains may be produced and, in point of fact, they are probably the only ways in which heavy rains are generally produced.

Applying the preceding principles to the case of the modern rain machine, let us inquire as to the probabilities of its successful operation. The simultaneous, or the successive explosion of large quantities of any high explosive, in the upper regions of the atmosphere, must produce, in general, a rapid and more or less thorough mixing or stirring of the surrounding air.

The sudden expansion of the air both by the heat liberated by the explosion and by the gases evolved during the explosion, is attended by a rush outward, followed by a rush of air inwards, towards the explosion centre. The direction of this latter rush is generally radially inward. In addition to these inward motions, the heat generated may tend to produce a slight upward motion; the general effect must be, however, to produce a mixing or churning rather than an upward motion.

The immediate effect of the explosion is to produce a miniature area of low barometer, caused by the radial rush of air towards the explosion centre, and by whatever ascending current that may result from the liberation of heat.

It would be reasonable to suppose that if the explosion produces any direct effect on atmospheric conditions, the area of low barometer should follow immediately, or nearly so, after the explosion. Have such changes in the barometric pressure been noticed to follow such mid-air explosion?

So far as the mixing motion is concerned, its action to produce a fall of rain must be slight. The ascending motion might cause a rainfall, but as this motion is slight in extent, its action under ordinary conditions, must at best be but insignificant.

In either case, any decrease in temperature, and consequent increase in relative humidity, must necessarily be slightly

decreased by the dry and heated gases evolved during the explosion of such substances as nitro-glycerine, dynamite or gunpowder.

It might be supposed from the above considerations that balloons containing an explosive mixture of hydrogen and oxygen would be preferable to those carrying nitro-glycerine dynamite or gunpowder, since in the former case the vapor of water results from the explosion, and in the latter dry gases. It must be remembered however, that the explosion of mixed oxygen and hydrogen produces, for the greater part, a collapse, or radial rush inwards *towards* the explosion centre, while the explosion of gunpowder or nitro-glycerine produces, for the greater part, a radial rush *from* such centre.

A circumstance that appears to have been lost sight of in all the recent attempts at rain-making, is that such attempts have been apparently made regardless of the hygrometric conditions of the air. As rain is but the excess of moisture, which the warm moist air when sufficiently cooled is unable to retain, the amount of the fall will depend, as already stated, on the quantity of moisture in the air as well as on the extent of the chilling action following the explosion or other cause. To attempt to produce rain by explosions in mid-air, irrespective of the quantity of moisture in the air, is to attempt to cause water to fall from the air when practically none is present. This is not only illogical but absurd.

It may be thought by some that the concussions caused by mid-air explosions might result in such a general movement of the surrounding air, as to cause rain to fall over an extended area. The flash of the explosion is followed by a sudden movement of the air causing the noise of the explosion. The phenomena of lightning and thunder are somewhat similar to those of artificial mid-air explosions. First we have the lightning flash, and subsequently the thunder which is a violent concussion of air. Does this concussion bring down a heavier rainfall? Popularly it is believed to do so, but the general opinion of the scientific world is that the lightning flash is the effect of a rapid condensation of

the aqueous vapor, *i. e.*, of a heavier rainfall, and not the cause of such a fall. That is to say the high potency of the lightning flash is due to the enormous decrease in the surfaces of the already charged rain drops over that of the surfaces of the thousands of the separate drops that coalesce to form the single drops.

Nevertheless, the liberation of heat energy and the rapid admixture of air following the disruptive discharge may slightly increase the rainfall, or may act as a determining cause of rain over an extended area.

There is this difference between the lightning flash and the flash of an explosion, viz: The former occurs over a comparatively great length of path, *i. e.*, a space of small breadth and depth but great length.

The latter occurs in a comparatively limited space, the three dimensions of which are nearly equal.

Though lightning is not a cause of rain, there can be no doubt that if rain can be artificially produced during a period in which there is much free electricity in the air, the storm will be attended by lightning and thunder. If then, there be any increase of rain due to the presence of lightning, artificial rain-making will be more liable to succeed when the potential of the air, as regards the earth or neighboring clouds, is comparatively high.

The enormous expenditure of energy required to produce a rain storm over an extended area is a circumstance that would appear to give but little encouragement to man's many efforts in this direction. The amount of energy liberated by the greatest explosion man has yet effected in mid-air, is but insignificant when compared to the energy liberated by nature during even a comparatively limited fall of rain.

There is, however, an important consideration, bearing on the question of the probable success of rain-making by mid-air explosions that gives to such attempts a far greater probability of success than would appear to be warranted from the facts already enumerated. Presupposing the existence of a sufficient mass of moist air, at preferably a comparatively high difference of potential

as compared with the neighboring air or the earth, a mid-air explosion might act as the determining cause of rainfall over a wide area, the balance of the energy requisite therefor, being supplied by the moist air. In a mass of very moist air there exists a store of energy which, if liberated, would suffice to cause movements of the air of vast extent. When the vapor of the air is condensed, the potential energy becomes kinetic, and, being liberated by the heat, causes ascending currents which produce a further condensation of moisture, and further liberation of energy previously locked up in the vapor.

There sometimes exist conditions in the air, in which it is, so to speak, in a state of very unstable equilibrium, and a slight determining cause may result in the liberation of the stored up energy with a resulting heavy rainfall. In such cases it would appear that there are no reasons why an explosion in mid-air should not be followed by rain. At the same time it is not unreasonable to suppose that the natural causes, which brought about such conditions, would, in many cases, continue to act and thus cause rain without artificial aid.

There are, however, meteorological conditions that probably frequently exist in certain latitudes in which heavy rains might be artificially produced by mid-air disturbances, when, without such disturbances, no rainfall would occur. Should, for example, a layer of warm, moist air exist between the earth's surface and a higher layer of cold, moist air, separated by a comparatively thin layer of air, and should such conditions exist as to maintain the two layers separate, then the breaking or piercing of the intermediate separating layer might permit such an up-rush of the warmer air through the opening, that the liberation of its stored up energy through the condensation of its moisture, would result in a general up-rush of the warm moist air and the consequent production of an extended area of low barometer. In other words, the artificial rupture of the separating layer would result in the formation of a true storm centre and a heavy rainfall of considerable dimensions. In such cases it would appear:

(1) That mid-air explosions will be more effective than explosions on the earth's surface.

(2) That directed mid-air explosions, *i. e.*, explosions in which the general effect of the liberated energy is to produce an upward rush of air, would be more effective than undirected, hap-hazard explosions.

If in such cases, considerable difference of potential exist between the layers of air, or between that of the air generally and the earth, the lightning flashes would unquestionably be effective in piercing the separating layer especially if, as would probably be the case, the general direction of the discharge be between the layers of cold and warm air.

Since, as we have seen, it is the ascending current that causes the heaviest rainfall, it would appear that mid-air explosions of such a character as to produce in general an upward rush of air would be probably more successful than undirected, hap-hazard explosions in mid-air. Such movements might advantageously be effected by the liberation of rockets with enlarged conical heads, or of any form of fire-work that would move generally upward.

Since success in artificial rain-making is probably dependent on the meteorological conditions, both of the lower and upper layers of the atmosphere, efforts should be made to enlarge our present very limited knowledge of such conditions.

Captive balloons, containing registering electrometers, tele-thermometers, tele-hygrometers, tele-anemometers, etc., might be connected by wires with recording apparatus placed on the earth's surface. The cost of maintaining such aërial stations of observation would be but insignificant when compared with the benefit that would accrue not only toward the solution of the problem as to the probable success in rain-making, but also in the aid given to the general operations of the United States Weather Bureau in particular, and to meteorology in general.

During the general prevalence of moist, warm air, when but a slight cooling is necessary to cause a general downpour, effective rain-making might be obtained by the sud-

den breaking or opening of cylinders of liquefied gases, where expansion would cause an intense chilling of the surrounding air; such cylinders could be readily opened by means of earth-controlled electro-magnets.

The following general conclusions may, in view of the present state of meteorological science, be properly drawn concerning the artificial production of rain.

(1) That rain can never be made to fall at will by mid-air explosions on any part of the earth's surface, irrespective of the climatic conditions there existing.

(2) That during certain meteorological conditions, mid-air explosions may result in rainfall over extended areas.

(3) That the liberation of energy necessary for such rain-falls is due not to the mid-air explosions, but to the energy stored up in the moist air from which the rain is derived.

(4) That the meteorological conditions which must exist for the successful action of mid-air explosions would probably, in most, though not in all cases, themselves result in a natural production of rain.

(5) That a comparatively high difference of electric potential between different parts of the air, or between the air and the earth, is possibly favorable when taken in connection with other meteorological conditions for artificial rain-making.

(6) That an undirected mid-air explosion is not as likely to produce rain, as an explosion in which the main tendency of the energy liberated is to cause a general up-rush of the air.

BOOK NOTICES.

Odontics; or, the Theory and Practice of the Teeth of Gears. By George B. Grant. Published by the Lexington Gear Works, Lexington, Mass., 1891. pp. 103.

This is called a working course of study, and the beginner is advised to first master certain sections, as the introduction says it is not necessary that the student, especially if he is a workman, should learn all that is taught in this book. Taking up the matter as directed is pretty hard work for any one, and it is doubtful whether a student would progress very far before deciding that he has had enough. For instance, the third paragraph introduces him to "axoids, "hyperboloid of revolution" and the next to "normal surfaces."

There is a radical defect in the work from a student's standpoint, which is not an uncommon one in books written by men full of their subject—and that is that the student is expected to take the author's statements as facts without proofs or reasons; for instance, in Section 11 the author states that "With the above conditions given we can deduce the following law: *The common normal to the tooth curves must pass through the pitch point.*" The conditions spoken of are not at all clearly set forth, and a student surely would like to know how such a law can be deduced. This is the author's method throughout the work. As to the parts to be taken after reading the easier portions it is even more difficult, as early in the book the reader is obliged to handle differential equations.

But the work is, nevertheless, an excellent one, as the matter in it is very valuable, and no one having some knowledge of gear wheels can fail to get new ideas from reading it.

The first part of the work is devoted to the general theory of tooth action, defining and explaining the terms used, the methods of constructing tooth curves by points, and of forming teeth mechanically.

The second part treats of spur gears, explaining circular and diametral pitch, giving actual sizes of various pitches, explaining and giving value for the addenda, clearance, etc., explaining the author's method of dividing a series of gears into intervals, each having the same shaped teeth, and determining the ruling size for each interval. The author also treats of the efficiency and strength of teeth and the power that can be transmitted.

In part third the subject of involute teeth is taken up, methods are given of laying down the gears and the applicability to symmetrical and unsymmetrical teeth, and numerous formulæ are given.

Part fourth treats of cycloidal teeth, the author paying his respects to the fossils who persist in using cycloidal teeth when involute are so much better. The subject is more satisfactorily presented than any other portion of the book. The descriptions and explanations are clear and to the point, and there is not too much of them.

The next part takes up the pin-tooth system, and is rather difficult to understand in some parts.

Part six treats of twisted, spiral and worm-gears; part seven of irregular and elliptic gears; part eight of bevel gears, and part nine of skew bevel gears. The matter in these parts is what an elementary work on gearing should contain, and excepting the small amount of mathematics can be readily handled by any one interested.

Throughout the work many of the mechanical details of actually laying out and manufacturing the gears are given, and the author has made use of about all the available experiments relating to efficiency, and pointed out the desirability of much more information in this direction.

As the matter was originally contributed to the *American Machinist* it is divided into short paragraphs under sub-heads, each paragraph extending over both columns of the page, thus making the book look and read disjointedly. There is, unfortunately, no index and no table of contents, so that it is inconvenient to refer to. H. W. S.

Car Lubrication. By W. E. Hall, B.S., M.E., New York: John Wiley & Sons, 53 East Tenth Street. 1891.

This little work, the outcome of recent studies by the author, is designed to throw light upon "some of the knotty problems which were then presented" to him.

The author recognizes the inapplicability of the ancient "three laws of friction" to lubricated surfaces; and, availing himself of the results of the modern investigations of Prof. Thurston and others, proceeds, in eminently practical fashion, to state the case as it appears to-day.

The theory of friction of lubricated surfaces is concisely stated, after which follow chapters devoted respectively to a review of experiments for determining coëfficients of friction for such surfaces, to the metals used in bearings and the oils for lubricating them, to methods of lubrication, to the construction of journal boxes, to the cost of lubrication and to the heating of journals.

The author inclines to skepticism as to the value of mechanical devices for improvement of lubricating methods, and believes that "the present method is by no means an imperfect one when the best is made of it."

T.

Retaining Walls for Earth. Including the Theory of Earth Pressure as Developed from the Ellipse of Stress. With an appendix presenting the Theory of Prof. Weyrauch. By Malverd A. Howe, C.E., Professor of Civil Engineering, Rose Polytechnic Institute. Second edition, revised and enlarged. New York: John Wiley & Sons, 53 East Tenth Street. 1891.

We quote from the preface:

"The first edition of this work was based upon the theory advanced by Prof. Weyrauch in 1878, but owing to the length of the demonstrations used by him, it was thought advisable to present different and shorter demonstrations in this edition. To show that the new demonstrations give identical results with those obtained by Prof. Weyrauch, his demonstrations have been given in an appendix as they appeared in the first edition.

"The new demonstrations are based upon the theory first advanced by Prof. Rankine in 1858."

Part I, of the present work, gives formulæ for earth-thrust, breadth of base of wall and depth of foundations, and Part II contains demonstrations of these formulæ. An appendix of four chapters gives Prof. Weyrauch's Theory of the Retaining Wall, taken from the *Zeitschrift für Baukunde*, references to authorities, a diagram for one of the values used in the formulæ, and tables of weights of masonry and backing, of angles and coëfficients of friction (from Rankine) of certain complex quantities used in the formulæ, and of natural trigonometrical functions.

The treatment, based as it is upon the works of Weyrauch and Rankine, is, of course, intensely mathematical, and no doubt of great interest from the scholastic point of view, and of value, too, notwithstanding the discussions among the professors as to what constitutes the true theory. To the "prac-

tical man," the usefulness of such studies must be prejudiced by the facts stated on p. 10, that a change of a few degrees in the value of the angle of repose sometimes causes a large change in that of the earth pressure, and that the former value must be "determined or assumed." In the language of an engineer of the last generation:

"The young engineer need not in practice concern himself particularly about the precise specific gravity of his backing, or about the angle of slope at which it will stand; for the material which he deposits behind his wall one day may be dry and incoherent, so as to slope at one and one-half to one; the next day rain may convert it into liquid mud, seeking its own level like water; the next it may be ice, capable of sustaining a considerable load, as a vertical pillar.

"Moreover, he cannot foretell what may be the nature of his backing; for as a general rule, this must consist of whatever the adjacent excavation may produce from time to time; sand to-day, rock to-morrow, etc. Retaining walls are, therefore, usually built before the engineer knows the character of their backing."

Hence, "the young engineer" may perhaps be pardoned for a feeling of modest hesitation in grappling with a proposition like equation (1), according to which, in the general case of inclined earth surface and inclined back of wall, the earth pressure is:

$$E = \frac{H^2 \gamma}{2} \frac{\cos(\varepsilon - a)}{\cos^2 a \cos \varepsilon}$$

$$\times \sqrt{\sin^2 a + \cos^2(\varepsilon - a) \left\{ \frac{\cos \varepsilon - \sqrt{\cos^2 \varepsilon - \cos^2 \phi}}{\cos \varepsilon + \sqrt{\cos^2 \varepsilon - \cos^2 \phi}} \right\}^2 + 2 \sin \varepsilon \sin a \cos(\varepsilon - a) \left\{ \frac{\cos \varepsilon - \sqrt{\cos^2 \varepsilon - \cos^2 \phi}}{\cos \varepsilon + \sqrt{\cos^2 \varepsilon - \cos^2 \phi}} \right\}}$$

in which ϕ is this very uncertain and variable angle of repose.

An admirable feature (a well-nigh indispensable one, indeed, in a work of this kind) is the list, preceding the discussion, of the symbols used, with their meanings. A similar list follows (why does it not also precede?) the account of Weyrauch's theories. T.

The Chronicle Fire Tables for 1891: A record of the fire losses in the United States by risks, States and causes during 1890, etc., and much other interesting and valuable information for fire underwriters. New York: The Chronicle Company, Limited. Price, $5.

This valuable and elaborate annual account of data, pertaining to fires in the United States, is fully equal to former issues, and gives evidence of immense labor in the gathering and correct presentation of the number,

causes, and the amounts of losses from fires during 1890. Each year as knowledge extends among city and town authorities, underwriters and manufacturers, of scientific efforts to understand, repress and combat fires, the greater is the desire, among an increasing number of persons, for such a series of reports as this one represents. Their value augments with the number of their years.

This volume contains 311 pages, royal 8vo, of fine paper with superior binding. Besides a general review of the fires occurring in the United States during 1890, there are presented nine full-page diagrams showing the movement in fire losses for sixteen years, and the monthly curves of incendiarism and its geographical distribution (by shading of States) for seven years, 1884–1890, inclusive. The principal tabulation of the volume gives the number of fires during 1890 by classes of risks, with property loss, insurance loss and causes of fires. There is presented an extended tabulation of fires during 1890 *by causes* (also a similar one *by States and Territories*) giving the number of fires, the exposures, property and insurance loss on original risks, property and insurance loss by exposures, with notes where fire originated.

The tabulations named will be sufficient to show the great labor required, the minutiæ of data and their consequent value; but there are others almost equal thereto, such as risks burned in the United States during the sixteen years, 1875–1890; monthly losses by fires, given by States and Territories; increase in the urban population of the United States; comparative table of fires and failures in business (very suggestive); losses by fire in each State and Territory during the fifteen years, 1876–1890; analysis of the causes of 171,139 fires in the United States, during six years, occasioning property loss, $670,952,891; aggregate annual fire losses in the United States for the sixteen years 1875–1890, etc.

We cannot too much commend this admirable statistical work. It is well worthy a place in the library of every insurance company in the world; also in that of every merchant, manufacturer and attorney-at-law. It will well repay careful study; and its cost is small compared with the expense of its production and the advantages it offers of knowledge on a most important subject, interesting every property-holder. N.

Franklin Institute.

[*Proceedings of the stated meeting, held Wednesday, Sept. 16, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, Sept. 16, 1891

Mr. W. P. TATHAM, Vice-President, in the chair.

Present, ninety members and eight visitors.

Additions to membership since last report, eleven.

On the recommendation of the Board of Managers, Prof. George Forbes, of London, England, was elected an honorary member of the Institute.

The Actuary reported a vacancy in the Board of Managers, caused by the death of Mr. John J. Weaver. Mr. John C. Trautwine, Jr., was thereupon elected to fill Mr. Weaver's unexpired term of office.

The Secretary reported that by the resignation of Mr. Wm. B. LeVan and the death of Mr. Louis H. Spellier, two vacancies had been created in the Committee on Science and the Arts. They were filled by the election of Mr. J. M. Emanuel for the unexpired term of Mr. LeVan, and of Mr. Spencer Fullerton for the unexpired term of Mr. Spellier.

The Secretary was directed to prepare suitable memorials of Mr. J. Luther Ringwalt and Mr. Louis H. Spellier, recently deceased members.

Lieut. Bradley A. Fiske, U.S.N., presented a paper on his "Range Finder," an instrument of precision, designed for finding the distance and position of an object, more especially of a vessel at sea. The paper was suitably illustrated by diagrams projected on the screen.

At the conclusion of the paper, Mr. H. R. Heyl, referring to the highly ingenious features embodied in the inventions, and to their obvious importance, moved that the subject be referred to the Committee on Science and the Arts for investigation and report. Carried. (The paper was referred for publication.)

Mr. C. J. Hexamer presented a brief communication, with illustrations, exhibiting a method that he had proposed and employed for verifying the manufacturing census of a city. The communication elicited some discussion between Mr. Henry Bower (Assistant in charge of the collection of the statistics of chemical manufactures for the eleventh census) and the author.

The Secretary's report contained some comments on current matters of technical interest, including a reference to a suite of interesting specimens illustrating the process of electric forging devised by Mr. Geo. D. Burton, of Boston.

Mr. W. N. Jennings presented a suite of lantern slides in continuation of his series of local pictures.

Adjourned.

WM. H. WAHL, *Secretary.*